N. Hadley Junr.

John Marmaduke Teesdale.

87655

Barr's Buffon.

Buffon's Natural History.

CONTAINING

A THEORY OF THE EARTH,
A GENERAL
HISTORY OF MAN,
OF THE BRUTE CREATION, AND OF
VEGETABLES, MINERALS,
&c. &c.

FROM THE FRENCH.
WITH NOTES BY THE TRANSLATOR.

IN TEN VOLUMES.

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CONTENTS.

OF

THE TENTH VOLUME.

Page

Of the Degeneration of Animals - 1

Nature and Properties of Minerals, Vegetables, &c.

Light, Heat, and Fire - - 27

Of Air, Water, and Earth - - 75

Experiments on the Progress of Heat in Mineral Substances - - - 109

A Table of the Relations of different Mineral Substances - - - 135

Observations on the Nature of Platina 156

Experiments on Light, and on the Heat it can produce.

Invention of Mirrors to burn at great distances - - - - 193

Observations and Experiments on Trees and other Vegetables - - 245

On the Temperature of the Planets - 279

General Views of Nature.

First View - - - 325

Second View - - - 343

BUFFON'S
NATURAL HISTORY.

OF THE DEGENERATION OF ANIMALS.

THE deer-kind whose horns are a sort of wood, and of a solid texture, although ruminating, and internally formed like those whose horns are hollow and porous, seem to form a separate family, in which the elk is the trunk, and the rein-deer, stag, axis, fallow-deer, and roe-buck, are the lesser and collateral branches; for there are only six species of animals whose heads are armed with branched horns that fall off and are renewed every year. Independently of this generic character, they resemble each other still more in formation and natural habitude; we should, therefore, sooner expect mules from the stag or fallow-deer, joined with the rein-deer or the axis, than from a union of the stag with the cow

We might be still better authorised to regard all the different kinds of sheep and goats as composing but one family, since they produce together mules, which immediately, and in the first generation, ascend to the species of sheep. We might even add to this numerous family of sheep and goats those of the gazelles and bubalus, which are not less in number. The muflon, the wild goat, the chamois, the antelope, the bubalus, the condoma, &c. seem to be the principal trunks of this genus, which contains more than thirty different species, and the others are only accessary branches which have retained the principal characters of the stocks from which they issued; but which, at the same time, have prodigiously varied by the influence of the climate, the difference of the food, and by the state of slavery to which man has reduced most animals.

The dog, the wolf, the fox, the jackal, and the isatis, form another genus, the different species of which resemble each other so strongly, especially in their internal conformation, and in the organs of generation, that it is difficult to conceive why they do not intermix. From the experiments which I made to form a union of the dog with the wolf and fox, the repugnance to copulate seemed to proceed from the wolf and fox rather than from the dog, that is, from the wild animal and not from the tame; for those bitches which I put to the trial would readily have permitted the wolf and fox, whereas as the females of the two latter would never suffer the approaches of the dog. The domestic state seems to render animals less faithful to their species: It gives them also a greater degree of heat and fecundity, for the bitch generally produces twice a year, while the females of the wolf and fox litter only once; and it is to be presumed, that those dogs which have been left in desert countries, and which have so greatly multiplied in the island of Juan Fernandes, and in the mountains of St. Domingo, &c. produce only once a year, like the wolf and the fox. This circumstance, if it were proved to be the fact, would fully establish the unity of genus in these three animals, which resemble each other in conformation so strongly as to oblige us to attribute their repugnance to some external circumstances.

The dog seems to be the intermediate species between the fox and the wolf. The ancients have stated, that the dog, in some countries, and under particular circumstances, engenders with the wolf and fox. I was desirous of verifying this assertion, and although I did not succeed in the trials I made, yet we must not conclude that it is impossible, for my experiments were with captive animals; and it is known that in some species captivity alone is sufficient to extinguish desire, and to give them a repugnance to copulation, even with their own kind; consequently they would still more refuse to unite with individuals of another species: but I am persuaded, that when in a state of freedom, and deprived of his own female, the dog would unite with the wolf and fox, particularly if he had become wild, lost his domestic cast, and approached the manner and natural habits of these animals. The fox and wolf, however, never unite, though they live in the same climate and country, but support their species pure and unmixed; we must, therefore, suppose a more ancient degeneration than history has recorded, if they ever belonged to one species; it was for this reason I asserted that the dog was an intermediate species between the fox and wolf; and his species is also common, since it can unite with both; and if any thing could shew that they all three originally sprang from the same stock, it is this common affinity between the dog, the fox, and the wolf, and which seems to bring their species nearer than all the conformities in their figures and organization. To reduce the fox and wolf, therefore, into one species, we must return to a state of nature very ancient indeed; but in their present condition, we must look upon the wolf and fox as the chief trunks in the genus of the five animals. The dog, the jackal, and the isatis, are only lateral branches placed between the two first; the jackal participates of the dog and wolf, and the isatis of the jackal and fox. From a great number of testimonies it appears that the jackal and the dog engender easily together; and it is observable, from the description and history of the isatis, that it almost entirely resembles the fox in its form and temperament, that they are equally found in cold countries, but that, at the same time, it inclines to the jackal in its disposition, continual barking, clamorous voice, and the habit of always going in packs.

The shepherd's dog, which I have considered as the original stock of every other dog, is, at the same time, that which approaches nearest in figure to the fox. He is of the same size, and, like the fox, he has erect ears, a pointed muzzle, and a strait trailing tail. He also approaches the fox in voice, sagacity, and instinct. The dog, therefore, may originally have been the issue of the fox, if not in a direct, at least in a collateral line. The dog, which Aristotle calls *canis-laconicus*, and which he affirms to have proceeded from an union of the fox and dog, might, possibly, be the same as the shepherd's dog, or, at least, it has more relation to him than to any other dog. We might, therefore, be inclined to imagine, that the epithet *laconicus*, left uninterpreted by Aristotle, was only given to this dog because he was found in Laconia, a province of Greece; and of which Lacedæmon was the capital; but if we attentively consider the origin of this *laconic* dog we shall perceive that the breed was not confined to the country of Laconia, alone but must have been found in every country where there were foxes; and this induces me to presume, that the epithet *laconicus* might possibly have been used by Aristotle in a moral sense, to express the brevity and acuteness of his voice, because he did not bark like other dogs, but had a shorter and shriller note, like that of the fox. Now our shepherd's dog is that to which we can justly apply this term of *laconic*, for of all dogs his voice is the sharpest and most rarely employed. Besides, the characters which Aristotle gives to his *laconic* dog agree with those of the shepherd's dog, and perfectly persuade me they are the same.

The genus of cruel and rapacious animals is one of the most numerous and most diversified; evils here, as in other cases, seem to be produced under every shape, and to assume various natures; the lion and the tiger, being detached species, rank in the first line; all the others, as the panther, the ounce, the leopard, the lynx, the caracal, the jaguar, the cougar, the ocelot, the serval, the margai, and the cat, compose only one cruel family, whose different branches are more or less extended and diversified according to the difference of climate. All these animals resemble each other in natural dispositions, although they are very different with respect to size and figure. They all have sparkling eyes, short muzzles, and sharp, crooked, and retractile claws. They are all destructive, ferocious, and untameable. The cat, which is the last and the least species, although reduced to slavery, continues its ferocity, and is no less perfidious. The wild cat has preserved the character of the family, and is as cruel and mischievous as any of his larger kindred. They are all equally carnivorous, and enemies to other animals. Man, with all his art and power, has not been able to annihilate them: fire, steel, poison, pits, and every method has been used against them without attaining that point. As the individuals are very prolific, and the species numerous, the efforts of man have been limited to keeping them at a distance, and confining them in the deserts, whence they never sally without spreading terror, and making great depredations. A single tiger issuing from the forest is sufficient to alarm a multitude of people, and oblige them to take up arms. What then would be the consequence if these sanguinary animals came in numbers, like wolves or jackals, to commit their depredations? Nature has given this instinct to timid animals, but fortunately denied it to the bold tribes; they go singly, and depend upon their courage and strength for their safety and support. Aristotle observed, and justly remarked, that of all animals furnished with talons not any of them are sociable, or go together in troops.* This observation, which was then confined to four or five species only, being all that were known in his time, is extended and verified over ten or twelve other species since discovered. Other carnivorous animals, such as the wolf, the fox, the dog, the jackal, and the isatis, whose claws are straight, go mostly in troops, and are all timid, and even cowardly.

By thus comparing every quadruped, and ranking each with its proper genus, we shall find, that the two hundred species of which we have given the history, may be reduced to a small number of families, or principal stems, from which it is not impossible all the others have derived their origin.

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* Nullum animal cui ungues adunci, gregatile esse perpendimus. Arist. Hist. Anim. Lib. i. Cap. 1.

To place this reduction in a regular method, we shall observe that all the animals of the two continents, as well as all those peculiar to the Old World, may be reduced to fifteen genera, and nine solitary species. These genera are, first, the whole hoofed genus, properly so called, which includes the horse, the zebra, and the ass, with all the prolific and barren mules. 2. The large cloven-hoofed with hollow horns, as the ox and the buffalo, with their varieties. 3. The small cloven-hoofed animals with hollow horns, such as the sheep, the goat, the gazelle, the antelope, and every other species which participates of their nature. 4. The cloven-hoofed with solid horns, which are shed and renewed every year; this family contains the elk, the rein-deer, the stag, the fallow-deer, the axis, and the roe-buck. 5. The ambiguous cloven-hoofed, which is composed of the wild boar, and all the varieties of the hog, such as that of Siam, with a hanging belly, that of Guinea, with long ears, pointed and turned backwards, and that of the Canary islands with thick and long tusks, &c. 6.

The very extensive race of digitated carnivorous animals with crooked and retractile claws, in which we must comprehend the panther, leopard, guepard, ounce, serval, and cat, with all their varieties. 7. The digitated carnivorous animals with straight and fixed claws, which include the wolf, fox, jackal, isatis, and the dog, with all their varieties. 8. The digitated carnivorous animals with fixed claws, and a pouch under their tails. This consists of the hyæna, civet, zibet, badger, &c. 9. The digitated carnivorous animals with long bodies, five toes to each foot, and the great toe, or thumb, divided from the rest; this genus is composed of the ferret, martin, pole-cat, weasel, sable, ichneumon, &c. 10. The numerous family of digitated quadrupeds which have two large incisive teeth in each jaw, and no bristles on their bodies; this contains the hare, rabbit, and every kind of squirrels, dormice, marmots, and rats. 11. The digitated quadrupeds, whose bodies are covered with spiny quills, as the porcupine and hedge-hog. 12. The digitated animals covered with scales, as the long and short-tailed manis, or scaly lizards. 13. The amphibious digitated genus, which includes the beaver, otter, musk-rats, walrus, and seals. 14. The four-handed genus, which comprehends the apes, baboons, monkies, makis, loris, &c. 15. The winged quadrupeds, which includes bats, &c. with all their varieties. The nine detached species are the elephant, rhinoceros, hippopotamus, giraffe, camel, lion, tiger, bear, and mole, which are all subject to a greater or smaller number of varieties.

Of those fifteen genera, and nine detached species, seven genera and two species are common to both continents. The two species are, the bear and the mole; and the seven genera are, 1. The great cloven-hoofed with hollow horns, for the ox is found in America, under the form of the bison. 2. The cloven-hoofed, with solid horns, for the elk exists in Canada, under the name of orignal; the rein-deer, under that of caribou; and stags, fallow-deer, and roe-bucks, are found in all the provinces of North America. 3. The digitated carnivorous animals with fixed claws; for the wolf and fox are found in the New World as well as in the Old. 4. The digitated animals with long bodies, as the weasel, martin, and pole-cat, are met with in America as well as in Europe. 5. We find also in America, part of the digitated genus with two large incisive teeth in each jaw, as the squirrels, marmots, rats, &c.

6. The digitated amphibious genus, as the walrus, seal, beaver, and otter, exist in the North of the New Continent. 7. The winged genus exist also in America, as the bat and vampire.

There remains, therefore, only eight genera, and five detached species, which are peculiar to the Old Continent. These eight genera are, 1. The whole-hoofed, properly so called, for neither the horse, ass, zebra, nor mule, were met with in the New Continent. 2. The small cloven-hoofed beasts with hollow horns; for sheep, goats, gazelles, or antelopes existed in America. 3. The family of hogs; for the species of wild boar is not to be found in America; and although the pecari, and its varieties, are related to this family, yet they differ in a sufficient number of remarkable characters to justify their separation. 4. It is the same with carnivorous animals with retractile claws; we do not meet with either the panther, leopard, guepard, ounce, or serval, in America; and although the jaguar, couguar, ocelot, and margai, seem to belong to this family, there is not one of these species of the New World found in the Old, nor one of the Old to be met with in the New. 5. The same remark may be applied to the digitated quadrupeds whose bodies are covered with prickles; for although the coendou and the urson approach very nigh to this genus, nevertheless, these species are very different from those of the porcupine and hedge-hog. 6. The digitated carnivorous genus with fixed claws, and a pouch under the tail; for the hyæna, civets, and the badger, do not exist in America. 7. The four-handed genus; for neither apes, baboons, monkeys, nor makis, have ever been seen in America. The sapajous, sagons, opossums, &c. although quadrumanous, yet they essentially differ from those of the Old Continent. 8. The digitated genus whose bodies are covered with scales; for none of the scaly lizards are found in America, and the anteaters, to whom they may be compared, are covered with hair, and differ too much from the scaly lizards to be considered of the same family.

Of the nine detached species, seven, namely, the elephant, rhinoceros, hippopotamus, giraffe, camel, lion, and tiger, are found only in the Old World; and two, viz. the bear and mole, are common to both continents.

If we, in the same manner, enumerate the animals which are peculiar to the New World, we shall find, that there are about fifteen different species which may be reduced to ten genera and four detached species. These four species are the tapir, the cabiai, the lama, and the pecari; but there is only the tapir we can absolutely term detached; for the pecari has varieties; and the pacos may be united to the lama, and the Guinea hog to the cabiai. The ten genera are, 1. Eight species of sapajous. 2. Six species of sagoins. 3. The opossums, phalangers, tarsiers, &c. 4. The jaguars, couguars, ocelots, margais, &c. 5. Three or four species of coatis. 6. Four or five species of mouffettes. 7. The agouti genus, which comprehends the acouchi, the paca, the aperea, and the tapeti. 8. That of the armadillos, which consists of seven or eight species. 9. Two or three species of ant-eaters; and, 10thly, The sloth, of which we are acquainted with but two species.

Now these ten genera, and four detached species, to which the fifty species of animals peculiar to the New World may be reduced, though they differ from those of the Old Continent, nevertheless have some relations which seem to indicate some common affinity in their formation, and lead us to causes of degeneration, more ancient than any of the rest. We have already made the general remark, that all animals of the New World were much smaller than those of the Old. This great diminution in size, whatever may be the cause, is a primary kind of degeneration, which could not be made without having a great influence on the figure of the animal, and we must not lose sight of this effect in comparing them together.

The largest is the tapir, which though not bigger than the ass, can only be compared with the elephant, rhinoceros, and hippopotamus; he claims the first place for size in the New Continent, as the elephant does in the Old. Like the rhinoceros, his upper lip is muscular and projecting; and, like the hippopotamus, he often enters the water. In some respects he represents them all three, and his figure, which partakes more of the ass than of any other animal, seems to be as degraded as his stature is diminished. The horse, the ass, the zebra, the elephant, the rhinoceros, and the hippopotamus, had no existence in America; neither was there an animal in this New Continent which could be compared with them, either with respect to size or figure. The tapir appears to have some affinity to the whale, but he is so mixed, and approaches so little to any one of them, that it is not possible to attribute his origin to the degradation of any particular species. And, notwithstanding these trifling relations which he is found to have with the rhinoceros, the hippopotamus, and the ass, we must look on him not only as a peculiar species, but even as a single genus.

The tapir, therefore, does not belong to any species of the Old Continent, and scarcely does he bear any characters which approximate him to those animals with which we have just been comparing him. The nature of the cabiai is likewise averse from our comparison: externally he has no resemblance with any other animal, and only approaches the Indian hog of the same continent, by his internal parts, and both species are absolutely different from all those of the Old Continent.

The lama and the pacos appear to have more significant marks of their ancient parents: the first with the camel, and the second in the sheep. The lama, like the camel, has a long neck and legs, slender head, and the upper lip divided. He resembles the latter also by his gentle manners, servility of disposition, endurance of thirst, and aptness for labour. This was the first and most useful domestic animal of the Americans: they made use of him to carry burdens, in the same manner as the Arabs do the camel. Here therefore are sufficient resemblances in the nature of these animals, to which we can yet add the permanent marks of labour; for though the back of the lama is not deformed by hunches like that of the camel, he, nevertheless, has callosities on his breast, occasioned by the like habit he is used to of resting on that part of his body. Yet, notwithstanding all these affinities, the lama is a very distinct and different species from the camel. He is much smaller, not exceeding a fourth or a third part of the camel's magnitude. The shape of his body, and the quality and colour of his hair, are also very different. His temperament is still more so; for he is a phlegmatic animal, and delights only to live on the mountains, whereas the camel is of a dry temperament, and willingly inhabits the most scorching sands. On the whole, there are more specific differences between the camel and the lama, than between the camel and the giraffe. These three animals have many characters in common, by which they might be referred to one genus, but, at the same time, they differ so much in other respects, that we cannot suppose them to be the issue of one another; they are, therefore, only neighbours and not relations. The height of the giraffe is nearly double that of the camel, and the camel double that of the lama. The two first belong to the Old Continent, and form separate species. The lama, therefore, which is only found in the New, must be a distinct species from both.

It is not the same with respect to the pecari, for though a different species from the hog, he, nevertheless, belongs to the same genus. He resembles the hog in shape, and every external appearance, and only differs from it in some trifling characters, such as the aperture on his back, shape of the stomach, intestines, &c. We might, therefore, be led to suppose that this animal sprung from the same stock as the hog, and that he formerly passed from the Old World to the New, where, by the influence of the soil, he had degenerated to so great a degree as now to constitute a distinct species.

With regard to the pacos, though it appears to have some affinities with the sheep, in its wool and habit of body, yet it differs so greatly in every other respect, that this species cannot be looked on either as neighbours or allies. The pacos is rather a small lama, and has not a single mark which indicates its having passed from one continent to the other. Thus of the four detached species peculiar to the New World, three, namely, the tapir, the cabiai, and the lama, with the pacos, appear to belong originally to this continent, whereas the pecari, which forms the fourth, seems to be only a degenerated species of the hog, and to have formerly derived its origin from the Old Continent.

By examining and comparing, in the same manner, the ten genera, to which we have reduced the other animals peculiar to South America, we shall discover, not only singular relations in their nature, but marks of their ancient origin and degeneration. The sapajous and sagoins bear so great a resemblance to the monkeys, that they are commonly included under that name. We have proved, however, that their species, and even their genera, are different. Besides, it would be very difficult to conceive how the monkeys of the Old Continent could assume in America a different-shaped visage, a long, muscular, and prehensile tail, a large partition between the nostrils, and other characters, both specific and generic, by which we have distinguished and separated them from the sapajous. But as the monkeys, apes, and baboons, are only found in the Old Continent, we must look upon the sapajous and sagoins as their representatives in the New, for these animals have nearly the same form, as well externally as internally, and also have many things in common in their natural habits and dispositions. It is the same with respect to the makis, none of which are found in America, yet they seem to be represented there by the opossums, or four-handed animals, with pointed muzzles, which are found in great numbers in the New Continent, but exist not in the Old. We must, however, observe, that there is much more difference between the nature and the form of the makis, and of these four-handed American animals, than between the monkeys and the sapajous; and that there is so great a distance between the opossums and the maki that we cannot form an idea that the one ever proceeded from the other, without supposing that degeneration can produce effects equal to those of a new nature; for the greatest number of these American four-handed animals have a pouch under the belly, ten incisive teeth in each jaw, and a prehensile tail; whereas the maki has a flaccid tail, no pouch under the belly, and only four incisive teeth in the upper jaw, and six in the lower; therefore, though all these animals have hands and fingers of the same form, and also resemble each other in the elongation of the muzzle, yet their species, and even their genera, are so different, that we cannot imagine them to be one and the same issue, or that such great and general disparities have ever been produced by degeneration.

On the other hand, the tigers of America, which we have indicated by the names of jaguars, couguars, ocelots, and margais, though different in species from the panther, leopard, ounce, guepard, and serval, of the Old Continent, are, nevertheless, of the same genera. All these animals greatly resemble each other, both externally and internally; they have also the same natural dispositions, the same ferocity, the same vehement thirst for blood, and what approximates them still nearer in genus, those which belong to the same continent differ more from each other than from those of the other Continent. For instance, the African panther differs less from the Brasilian jaguar than the latter does from the couguar, though they are natives of the same country. The Asiatic serval, and the margai of Guiana, likewise differ less from one another than from the species peculiar to their own continents. We, therefore, may justly suppose, that these animals had one common origin, and that, having formerly passed from one continent to the other, their present differences have proceeded only from the long influence of their new situation. The mouffettes, or stinkards, of America, and the polecat of Europe, seem to be of the same genus. In general, when a genus is common to both continents the species which compose it are more numerous in the Old than in the New; but in this instance it is quite the reverse, for there are four or five kinds of polecats in America, while we have only one, the nature of which is inferior to that of all the rest; so that the New World, in its turn, seems to have representatives in the Old; and if we judged only from the fact, we might think these animals had taken the opposite road, and passed from America to Europe. It is the same with respect to some other species. The roebucks and the fallow-deer, as well as the stinkards, are more numerous, larger, and stronger in the New Continent than in the Old; we might, therefore, imagine them to be originally natives of America; but as we cannot doubt that every animal was created in the Old Continent, we must, consequently, admit of their migration from the Old to the New World, and at the same time suppose, that instead of having degenerated, like other animals, they have improved their original nature by the influence of the soil and climate.

The ant-eaters, which are singular animals, and of which there are three or four species in the New World, seem also to have their representatives in the Old. The scaly lizards resemble them in the peculiar character of having no teeth, and of being obliged to put out their tongues and feed upon ants; but if we would suppose them to have one common origin, it is strange, that instead of scales, with which they are covered in Asia, they are clothed with hair in America.

With respect to the agoutis, pacos, and other animals of the seventh genus peculiar to the New Continent, we can only compare them with the hare and rabbit, from which, however, they all differ in species. What renders their being of a common origin doubtful is, the hare being dispersed almost over every climate of the Old Continent, without having undergone any other alteration than in the colour of its hair. We cannot, with any foundation, therefore, imagine that the climate of America has so far changed the nature of our hares to so great a degree as to make them tapetis or apereas, which have no tail; or agoutis with pointed muzzles, and short round ears; or pacos, with a large head, short ears, and a coarse hair marked with white stripes.

On the whole, the coatis, the armadillos, and the sloths, are so different, not only in species, but also in genus, from every animal of the Old World, that we cannot compare them with any one; it is also impossible to refer them to any common origin, or attribute to the effects of degeneration the prodigious differences found in their nature from that of every other animal.

Thus, of ten genera, and four detached species, to which we have endeavoured to reduce all the animals peculiar to the New World, there are only two, the genus of the jaguars, ocelots, &c. and the species of the pecari, with their varieties, which can with any foundation be connected with the animals of the Old Continent. The jaguars and ocelots may be regarded as a species of the leopard or panther, and the pecari as a species of hog. After these are five genera and one detached species, namely the species of the lama, and the genera of sapajous, sagoins, stinkards, agoutis, and ant-eaters, which may be compared, though in a very distant and equivocal manner, with the camel, monkey, polecat, hare, and scaly lizards. There then remain four genera and two detached species, namely, the opossums, the coatis, the armadillos, the sloths, the tapir, and the cabiai, which can neither be referred nor compared to any genera or species of the Old Continent. This sufficiently proves that the origin of these animals, peculiar to the New world, cannot be attributed merely to degeneration. However, great and powerful the effects of degeneration may be supposed, we cannot, with any appearance of reason, persuade ourselves that these animals were originally the same as those of the Old Continent. It is more reasonable to imagine that the two continents were formerly joined, and that those species which inhabited the New World, because they found the climate and soil most suitable to their nature, were separated from the rest by the irruption of the sea when it divided Asia from America. This is a natural cause, and similar ones might be conceived which would produce the same effect; for example, if the sea should make an irruption from the eastern to the western side of Asia, and thus separate the southern parts of Africa and Asia from the rest of the Continent, all the animals peculiar to the southern countries, such as the elephant, the rhinoceros, the giraffe, the zebra, the orang-outang, &c. would be, relatively to the others, the same as those of South America at present are; they would be entirely separated from the animals of the temperate countries, and could not be referred to an origin common to any of the species or genera which inhabit these countries, on the sole foundation that some imperfect resemblances, or distant relations, might be observed between them.

We must, therefore, to find out the origin of these animals, turn back to the time when the two continents were not separated, and refer to the first changes which happened on the surface of the globe. We must, at the same time, place before our view the two hundred species of quadrupeds as constituting thirty-eight families; and although this is not the state of nature, such as it is come down to us, and as we have represented it, but, on the contrary, a much more ancient state, which we can only attain by inductions and relations nearly as fugitive as time, which seems to have effaced their traces, we have endeavoured, by facts and monuments still existing, to return to those first ages of nature, and to exhibit those epochas which appear to be most clearly indicated.

NATURE AND PROPERTIES OF MINERALS, VEGETABLES, &c.

LIGHT, HEAT, AND FIRE.

ALL the powers of Nature with which we are acquainted, may be reduced to two primitive forces; the one which causes weight, and that which produces heat. The force of impulsion is subordinate to them; it depends on the first for its particular, and on the latter for its general effects. As impulsion cannot exercise itself but by the means of a spring, and the spring only acts by virtue of the force which approximates the remote parts, it is clear, that to perform its power it has need of the concurrence of attraction: for if matter ceased to attract, if bodies lost their coherence, every spring would be destroyed, every motion intercepted, and every impulsion void; since motion cannot transmit itself from one body to another but by elasticity, it is demonstrable, that one body absolutely hard and inflexible, would be absolutely immoveable, and entirely incapable of receiving the action of another. Attraction being a general and permanent effect, impulsion, which in most bodies is neither constant nor fixed, depends on it as a particular effect; for, if all impulsion were destroyed, attraction would still equally subsist and act; it is, therefore, this essential difference which makes impulsion subordinate to attraction in all inanimate and purely passive matter.

But this impulsion depends still more immediately, and generally, on the power which produces heat; for it is principally by the means of heat, that impulsion penetrates organized bodies; it is by heat that they are formed, grow, and develope themselves. We may refer to attraction alone all the effects of inanimate matter; and in this same power of attraction, joined to that of heat, every phenomena of live matter. By live matter I understand not only every thing that lives, or vegetates, but also every living organic molecule, dispersed in the waste or remains of organized bodies. In it I comprehend also light, heat, fire, and all matter which appears to be active in itself. Now this live matter always tends from the centre to the circumference, whereas brute or inanimate matter tends from the circumference to the centre. It is an expansive power which animates the live matter, and it is an attractive force to which the inanimate matter is obedient. Although the directions of these two powers be diametrically opposite, yet they balance themselves without ever being destroyed, and from the combination of these two powers equally active, all the phenomena of the universe result.

But it may be said, by reducing all the powers of Nature to attraction and expansion, without giving the cause of either, and by rendering impulsion, (which is the only force whose cause is known and demonstrated to our senses) subordinate to both, do you not abandon a clear idea, and substitute two obscure hypotheses in its place? To this I answer, that as we know nothing except by comparison, we shall never have an idea of what general effect will produce, because such an effect belonging to every thing, we should be unable to compare it to any, and consequently there is no hope of ever knowing the cause or reason why all matter attracts, although we are sensible such is the fact. If, on the contrary, the effect were particular, like that of the attraction of the loadstone and steel, we might expect to discover the cause, because it might be compared to other particular effects. To ask why matter is extended, heavy, and impenetrable, are ill-conceived propositions, and merit not an answer; it is the same with respect to every particular property, when it is essential to the subject, and we might as well be interrogated why red is red? The philosopher becomes a child when he puts such questions; and however much they may be forgiven to the last, the former ought to exclude them from his thoughts.

It is sufficient that the forces of attraction and expansion are two general, real, and fixed effects, for us to receive them for causes of particular ones; and impulsion is one of these effects, which we must not look upon as a general cause, known and demonstrated by our senses, since we have proved that this force of impulsion cannot exist nor act, but by the means of attraction, which does not fall upon our senses. Nothing is more evident, nav, certain, than the communication of motion by impulsion; it is sufficient for one body to strike another to produce this effect. But even in this sense, is not the cause of attraction most evident, and that motion, in all cases, belongs more to attraction than impulsion?

The first reduction being made, it might perhaps be possible to adduce a second, and to bring back the power even of expansion to that of attraction, insomuch that all the forces of matter would depend solely on a primitive one; at least this idea seems to be worthy of that sublime simplicity with which nature works. Now cannot we conceive that this attraction changes into repulsion every time that bodies approach near enough to rub together, or strike one against the other? Impenetrability, which we must not regard as a force, but as a resistance essential to matter, not permitting two bodies to occupy the same place, what must happen when two molecules, which attract the more powerfully as they approach nearer, suddenly strike against each other? Does not then this invincible resistance of impenetrability, become an active force, which, in the contact, drives the bodies with as much velocity, as they had acquired at the moment they touched? And from hence the expansive force will not be a particular force opposed to the attractive one, but an effect derived therefrom. I own, that we must suppose a perfect spring in every molecule, and in every atom of matter, to have a clear conception how this change of attraction into repulsion is performed. But even this is sufficiently indicated by facts; the more matter is attenuated, the more it takes a spring. Earth and water, which are the most gross aggregates, have a less spring than air; and fire, which is the most subtle of all the elements, is also that which has the most expansive force. The smallest molecules of matter, the smallest atoms with which we are acquainted are those of light, and we are sensible of their being perfectly elastic, since the angle under which the light is reflected, is always equal to that under which it comes. We may therefore infer, that all the constitutive parts of matter in general, are a perfect spring; and that this spring produces all the effects of the expansive force, every time that bodies strike by meeting in opposite directions.

We know of no other means of producing fire, but by striking or rubbing bodies together*; since by supposing man without any burning glasses, and without actual fire, he will have no other means of producing it; for the fire produced by uniting the rays of light, or by application of fire already produced, had the same origin.

Expansive force, therefore, in reality might be only the re-action of the attractive, a re-action which operates every time that the primitive molecules of matter, always attracted one by the other, happen immediately to touch; for then it is necessary, that they be repelled with as much velocity as they had acquired in a contrary direction, at the mo-moment of contact; and when these molecules are absolutely free from all coherence and only obey the motion alone produced by their attraction, this acquired velocity is immense in the point of contact. Heat, light, and fire, which are the greatest effects of expansive force, will be produced every time that bodies are either artificially or naturally divided into very minute parts, and meet in opposite directions; and the heat will be so much the more sensible, the light so much the more bright, the fire so much the more violent, according as the molecules are precipitated one against the other with more velocity by their force of mutual attraction.

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* The fire, which arises from the fermentation of herbs heaped together, and which manifests itself in effervescences, is not an exception that can be opposed to me, since this production of fire depends, like all the rest, from the action of the shock of the parts of matter one against the other.

From the above it must be concluded, that all matter may become light, heat, and fire; and that this matter of fire and light is not a substance different from every other, but preserves all its essential qualities; and even most of the attributes of common matter, is evidently proved by, first, light, though composed of particles almost infinitely minute, is, nevertheless, still divisible, since with the prism we separate the rays, or different coloured atoms one from another. Secondly, light, though in appearance endowed with a quality quite opposite to that of weight, that is, with a volatility which we might think essential, is, nevertheless, heavy like all matter, since it bends every time it passes near other bodies, and finds itself inclined to their sphere of attraction. It is very heavy, relatively to its volume, which is very minute, since the immense velocity with which light moves in a direct line, does not prevent it from feeling sufficient attraction near other bodies, for its direction to incline and change in a manner very sensible to our eyes. Thirdly, the substance of light is not more simple than all other matter, since it is composed of parts of unequal weight; the red rays are much heavier than the blue; and between these two extremes there are an infinity of intermediate rays, which approach more or less the weight of the red, or the lightness of the blue according to their shades. All these consequences are necessarily derived from the phenomena of the inflection of light, and of its refraction, which, in reality, is only an inflexion which operates when light passes across transparent bodies. Fourthly, it may be demonstrated, that light is massive, and that it acts, in some cases, as all other bodies act; for, independently of its ordinary effect, which is to shine before our eyes, and by its own action, always accompanied with lustre, and often with heat, it acts by its mass when it is condensed, and it acts to the point of putting in motion heavy bodies placed in the focus of a good burning glass: it turns a needle on a pivot placed in its focus: it displaces leaves of gold or silver before it melts or even sensibly heats them. This action, produced by its mass, precedes that of heat: it operates between the condensed light and the leaves of metal in the same manner as it operates between two other bodies which become contiguous, and, consequently, have still this property in common with all other matter. Fifthly, light is a mixture, like common matter, not only of more gross and minute parts, more or less heavy or moveable, but also differently shaped. Whoever has observed the phenomena which Newton calls *the access of easy reflection*, and of *easy transmission of light*; and on the effects of double refraction of rock and Iceland chrystal, must have perceived that the atoms of light have many sides, many different surfaces, which, according as they present themselves, constantly produce different effects.

This, therefore, is sufficient to demonstrate that light is neither particular nor different from common matter; that its essence, and its essential properties are the same; and that it differs only from having undergone, in the point of contact, the repulsion whence its volatility proceeds; and in the same manner as the effect of the force of attraction extends, always decreasing as the space augments, the effects of repulsion extend and decrease the more, but in an inverted order, insomuch that we can apply to the expansive force all that is known of the attractive. These are two instruments of the same nature, or rather the same instrument, only managed in two opposite directions.

All matter will become light, for if all coherence were destroyed it would be divided into molecules sufficiently minute, and these molecules, being at liberty, will be determined by their mutual attraction to rush one against the other. In the moment of the shock the repulsive force will be exercised, the molecules will fly in all directions with an almost infinite volatility, which, nevertheless, is not equal to their velocity acquired in the moment of contact, for the law of attraction being augmented as the space diminishes, it is evident, that at the contact the space is always proportionable till the square of the distance becomes nil, and, consequently, the velocity acquired by virtue of the attraction must at this point become almost infinite: and it would be perfectly so if the contact were immediate, and, consequently, the distance between the two bodies void; but there is nothing in nature entirely nil, and nothing truly infinite; and all that I have observed of the *infinite* minuteness of the atoms which constitute light, of their *perfect* spring, and of the *nil* distance in the moment of contact, must be understood only relatively. If this metaphysical truth were doubted, a physical demonstration may be given. It is pretty generally known that light employs seven minutes and a half to come from the sun to the earth; supposing, therefore, the sun at thirty-six millions of miles, light darts through this enormous distance in that short space, that is (supposing its motion uniform), 80,000 miles in one second. But this velocity, although prodigious, is yet far from being infinite, since it is determinable by numbers. It will even cease to appear so prodigious, when we reflect on the celerity of the motion of the comets to their perihelia, or even that of the planets, and by computing that, we shall find that the velocity of those immense masses may pretty nearly be compared to that of the atoms of light.

So, likewise, as all matter can be converted into light by the division and expulsion of its parts, when they feel a shock one against another, we shall find that all the elements are convertible; and if it have been doubted whether light, which appears to be the most simple element, may be converted into a solid substance, it is because we have not paid sufficient attention to every phenomena, and were infected with the prejudice, that being essentially volatile it can never become fixed. But it is plain that the fixity and volatility depend on the same attractive force in the first case, and become repulsive in the second; and from thence are we led to think that this change of matter into light, and from light into matter, is one of the most frequent operations of Nature.

Having shewn that impulsion depends on attraction; that the expansive force, like the attractive, becomes negative; that light, heat, and fire, are only modes of the common existing matter; in one word, that there exists but one sole force, and one sole matter, ever ready to attract or repel, according to circumstances; let us see how, with this single spring, and this single subject, Nature can vary her works, *ad infinitum*. In a general point of view, light, heat, and fire, only make one object, but in a particular point of view they are three distinct objects, which, although resembling in a great number of properties, differ nevertheless in a few others, sufficiently essential for us to consider them as three distinct things.

Light, and elementary fire, compose, it is said, only one and the same thing. This may be, but as we have not yet a clear idea of elementary fire we shall desist from pronouncing on this first point. Light and fire, such as we are acquainted with, are two distinct substances, differently composed. Fire is, in fact, very often luminous, but it sometimes also exists without any appearance of light. Fire, whether luminous or obscure, never exists without a great heat, whereas light often burns with a noise without the least sensible heat. Light appears to be the work of nature while fire is only the produce of the industry of man. Light subsists of itself, and is found diffused in the immense space of the whole universe. Fire cannot subsist without food, and is only found in some parts of this space where man preserves it, and in some parts of the profundity of the earth, where it is also supported by suitable food. Light when condensed and united by the art of man, may produce fire, but it is only as much as it lets fall on combustible matters. Light is therefore no more, and in this single instance, only the principle of fire and not the fire itself: even this principle is not immediate, for it supposes the intermediate one of heat, and which appears to appertain more than light to the essence of fire. Now heat exists as often without light as light exists without heat: these two principles might, therefore, appear not to bind them necessarily together; their effects are not contemporary, since in certain circumstances we feel heat long before light appears, and in others we see light long before we feel any heat. Hence is not heat a mode of being, a modification of matter, which, in fact, differs less than all the rest from that of light, but which can be considered apart, and still more easily conceived? It is, nevertheless, certain, that much fewer discoveries have been made on the nature of heat than on that of light; whether man better catches what he sees than what he feels; whether light, presenting itself generally as a distinct and different substance from all the rest, has appeared worthy of a particular consideration; whereas heat, the effect of which is the most obscure, and presents itself as a less detached and less simple object, has not been regarded as a distinct substance,

substance but as an attribute of light and fire.

The first thing worthy of remark, is, that the seat of heat is quite different from that of light: the latter occupies and runs through the void space of the universe; heat, on the contrary, is diffused through all solid matter. The globe of the earth, and the whole matter of which it is composed, have a considerable degree of heat. Water has its degree of heat which it does not lose but by losing its fluidity. The air has also heat, which we call its temperature, and which varies much, but is never entirely lost, since its springs subsist even in the greatest cold. Fire has also its different degrees of heat, which appear to depend less on its own nature, than on that of the aliments which feed it. Thus all known matter possesses warmth; and, hence, heat is a much more general affection than that of light.

Heat penetrates every body without exception which is exposed to it, while light passes through transparent bodies only, and is stopped and in part repelled, by every opaque one. Heat, therefore acts in a much more general and palpable manner than light, and although the molecules of heat are excessively minute, since they penetrate the most compact bodies, it seems, however, demonstrable, that they are much more gross than those of light; for we make heat with light, by collecting it in a great quantity. Besides, heat acting on the sense of feeling, it is nececssary that its action be proportionate to the grossness of this sense, the same as the delicacy of the organs of sight appears to be to the extreme fineness of the parts of light; these parts move with the greatest velocity, and act in the instant at immense distances, whereas those of heat have but a slow progressive motion, and only extend to small intervals from the bodies whence they emanate.

The principle of all heat seems to be the attrition of bodies; all friction, that is, all contrary motion between solid matters produces heat; and if the same effect do not happen to fluids, it is because their parts do not touch close enough to rub one against the other; and that, having little adherence between them, their resistance to the shock of other bodies is too weak for the heat to be produced to a sensible degree; but we often see light produced by an attrition of a fluid, without feeling any heat. All bodies whether great or little become heated as soon as they meet in a contrary direction; heat is, therefore, produced by the motion of all palpable matter;

while the production of light, which is also made by motion, but in a contrary direction, supposes also the division of matter into very minute parts: and as this operation of Nature is the same with respect to both, we must conclude, that the atoms of light are solid of themselves, and are hot at the moment of their birth. But we cannot be equally certain, that they preserve their heat in the same degree as their light, nor that they cease to be hot before they cease to be luminous.

It is well known, that heat grows less, or cold becomes greater, the higher we ascend on the mountains. It is true that the heat which proceeds from the terrestrial globe, is of course sensibly less on those advanced points, than it is on the plains; but this cause is not proportionable to the effect; the action of heat, which emanates from the terrestrial globe, not being able to diminish but by the square of the distance, it does not appear that at the height of half a mile, which is only the three thousandth part of the semi-diameter of the globe, whose centre must be taken for the focus of heat, that this difference, which in this supposition is only a unit and nine millions, can produce a diminution of heat nearly so considerable; for the thermometer lowers at that height, at all times of the year, to the freezing point. It is not probable, that this great difference of heat simply proceeds from the difference of the earth; and of that we must be fully convinced, if we consider, that at the mouth of the volcanos, where the earth is hotter than in any other part on the surface of the globe, the air is nearly as cold as on other mountains of the same height.

It may then be supposed that the atoms of light, though very hot at the moment of quitting the sun, are greatly cooled during the seven minutes and a half in which they pass from that body to the earth; and this in fact would be the case if they were detached; but, as they almost immediately succeed each other, and are the more confined as they are nearer the place of their origin, the heat lost by each atom falls on the neighbouring ones; and this reciprocal communication supports the general heat of light a longer time; and as their constant direction is in divergent rays, their distance from each other increases according to the space they run over; and as the heat which flies from each atom, as a centre, diminishes also in the same ratio, it follows, that the light of the solar rays, decreasing in an inverted ratio from the square of the distance, that of their heat decreases in an inverted ratio of the square of the same distance.

Taking therefore the semi-diameter of the sun for a unit, and supposing the action of light to be as 1000 to the distance of a demi-diameter of the surface of this planet, it will not be more than as $\frac{1000}{4}$ to the distance of two demi-diameters; as $\frac{1000}{9}$ to that of three demi-diameters, as $\frac{1000}{16}$ to the distance of four demi-diameters; and finally, when it arrives at us, who are distant from the sun thirty-six millions of leagues, that is about two hundred and twenty-four of its demi-diameters, the action of light will be no more than as $\frac{1000}{50625}$, that is, more than 50,000 times weaker than at its issuing from the sun; and the heat of each atom of light being also supposed 1000 at its issuing from the sun, will not be more than as $\frac{1000}{16}$ $\frac{1000}{81}$ $\frac{1000}{256}$ to the successive of 1, 2, 3, demi-diameters, and, when arrived at us, as $\frac{1000}{2562890625}$ that is, more than two thousand five hundred millions of times weaker than at issuing from the sun.

If even this diminution of the heat of light should not be admitted by reason of the squared square of the distance to the sun, it will still be evident that heat, in its propagation, diminishes more than light. If we excite a very strong heat, by kindling a large fire, we shall only feel it at a moderate distance but we shall see the light at a very great one. If we bring our hands by degrees nearer and nearer a body excessively hot, we shall perceive that the heat increases much more in proportion than as the space diminishes; for we may warm ourselves with pleasure at a distance which differs only by a few inches from that at which we should be burnt. Every thing, therefore, appears to indicate, that heat diminishes in a greater ratio than light, in proportion as both are removed from the focus whence they issued.

This might lead us to imagine, that the atoms of light would be very cold when they came to the surface of our atmosphere; but that by traversing the great extent of this transparent mass, they receive a new heat by friction. The infinite velocity with which the particles of light rub against those of the air, must produce a heat so much the stronger as the friction is more multiplied: and it is, probably, for this reason, that the heat of the solar rays is found much stronger in the lower parts of the atmosphere, and that the coldness of the air appears to augment as we are elevated. Perhaps, likewise, as light receives heat only by uniting, a great number of atoms of light is required to constitute a single atom of heat, and this may be the cause why the feeble light of the moon, although in the atmosphere, like that of the sun, does not receive any sensible degree of heat. If, as M. Bouguer says, the intensity of the light of the sun to the surface of the earth is 300,000 times stronger than that of the moon, the latter must be almost insensible, even by uniting it in the focus of the most powerful burning glasses, which cannot condense it more than 2000 times; subtracting the half of which for the loss by reflexion or refraction, there remains only a 300dth part intensity to the focus of the glass.

Thus, we must not infer that light can exist without any heat, but only that the degrees of this heat are very different, according to different circumstances, and always insensible when light is very weak. Heat, on the contrary, seems to exist habitually, and even to cause itself to be strongly felt without light; for in general it is only when it becomes excessive, that light accompanies it. But the very essential difference between these two modifications of matter is, that heat, which penetrates all bodies, does not appear to fix in any one, whereas light incorporates and extinguishes in all those which do not reflect, or permit it to pass freely; heat bodies of all kinds to any degree, in a very short time they will lose the acquired heat, and return to the general temperature. If we receive light on black or white bodies, rude or polished, it will easily be perceived, that some admit, and others repe it; and that instead of being affected in a uniform manner as they are by heat, they are only so relatively to their nature, colour, and polish. Black will absorb more light than white, and the rough more than the smooth. Light once absorbed remains fixed in the body which received it, nor quits it like heat; whence we must conclude, that atoms of light may become constituent parts of bodies by uniting with the matter which composes them; whereas heat not fixing at all, seems to prevent the union of every part of matter, and only acts to keep them separate. Nevertheless, there are instances where heat remains fixed in bodies, and others where the light they have absorbed re-appears, and goes out like heat.

After all there appear to be two kinds of heat, the one luminous, of which the sun is the focus; the other obscure, of which the grand reservoir is the terrestrial globe. Our body, as making part of the globe, participates of this obscure heat; and it is for this reason, that it is still obscure to us, because we do not perceive it by any one of our senses. It is with respect to this heat of the globe, as with its motion, we are subject to and participate thereof without feeling or doubting of it: from hence it happened that physicians at first carried all their views and enquiries on the heat of the sun, without suspecting that it makes but a very small part of what we really feel; but having made instruments to discover the difference of the immediate heat of the rays of the sun, they with astonishment found that the heat of them was sixty-six times stronger in summer than in winter, notwithstanding the strongest heat of our summer differs only a seventh from the strongest cold of our winter; from whence they have concluded, that, independent of the heat we receive from the sun, there emanates another, even from this terrestrial globe, which is much more considerable; insomuch, that it is at present demonstrable, that this heat, which escapes from the bowels of the earth, is in our climate at least twenty-nine times in summer, and four hundred times in winter, stronger than the heat which comes to us from the sun.

This strong heat which resides in the interior part of the globe, and which, without ceasing to emanate externally, must, like an element, enter into the combination of all the other elements. If the sun is the parent of Nature, the heat of the earth must be the mother; they both unite to produce, support, and animate organized beings, and to assimilate and compose inanimate substances. This internal heat of the globe, which tends always from the centre to the circumference, is, in my opinion, a great agent in nature. We can scarcely doubt but it is the principal influence on the perpendicularity of the trunks of trees, on the phenomena of electricity, on the effects of magnetism, &c. But as I do not pretend to make a physical treatise here, I shall confine myself to the effects of this heat on the other elements. It is alone sufficient to maintain the rarefaction of the air to the degree that we breathe in: it is more than sufficient to keep water in its state of fluidity, for we have lowered the thermometers to the depth of 120 fathoms, and have found the temperature of the water was there nearly the same as at the like depth in the earth, namely, ten degrees two thirds. We must not, therefore, be surprized, especially as salt acts as a prevention, that the sea in general does not freeze, that fresh water freezes but to a certain thickness, and that the water at bottom always remains liquid, even in the most intense frosts.

But of all the elements the earth is that on which this internal heat must necessarily have produced, and still produces the greatest effects. This heat originally was doubtless much greater than it is at present; therefore we must refer to it, as to the first cause, all the sublimations, precipitations, aggregations, and separations, which have been, and still continue to be made in the internal part of the globe, especially in the external layer which we have penetrated, and the matter of which has been removed by the convulsions of Nature, or by the hands of man. The whole mass of the globe having been melted, or liquefied, by fire, the internal is only a concrete or discreet glass, whose simple substance cannot receive any alteration by heat alone; there is, therefore, only an upper and superficial layer, which being exposed to the action of external causes united to that of the internal heat, will have undergone all the modifications, differences, and forms, in one word, of Mineral Substances, which their combined actions were enabled to produce.

Fire, which at first sight appears to be only a compound of heat and light, might also be a modification of the matter, though it does not essentially differ from either, and still less from both taken together. Fire never exists without heat, but it can exist without light. Heat alone, deprived of all appearance of light, can produce the same effects as the most violent fire; so can also light, when it is united. Light seems to carry a substance in itself which has no need of fuel; but fire cannot subsist without absorbing the air, and it becomes more violent in proportion to the quantity it absorbs; whereas light, concentrated and received into a vessel exhausted of air, acts as fire in air; and heat, confined and retained in a narrow space, subsists and even augments with a very small quantity of food. The most general difference between fire, heat, and light, appears, therefore, to consist in the quantity, and perhaps quality, of their food.

Air is the first food of fire; combustible matters are only the second. It has been demonstrated, by experiments, that a little spark of fire, placed in a vessel well closed, in a short time absorbs a great quantity of air, and becomes extinguished as soon as the quantity or quality, of this food becomes deficient. By other experiments it is proved, that the most combustible matters will not consume in vessels well closed, although exposed to the action of the greatest fire. Air is, therefore, the first and true food of fire, and combustible matters would not be able to supply it without the assistance and mediation of this element.

We have observed that heat is the cause of all fluidity, and we find, by comparing some fluids together, that more heat is requisite to keep iron in fusion than gold; and more to keep gold than tin; much less is necessary for wax, for water less than that, and still less for spirits of wine, and a mere trifle is sufficient for mercury, since the latter goes 187 degrees below what water can without losing its fluidity; mercury, therefore, is the most fluid of all matter, air excepted. Now this superior fluidity in air indicates the least degree of adherence possible between its constituting parts, and supposes them of such a figure as only to be touched at one point. It may be also imagined, that, being endowed with so little apparent energy and mutual attraction, they are, for that reason, less massive, and more light, than those of every other body; but that conclusion appears unfounded, from the comparison of mercury, the next fluid body, but of which the constituting parts appear to be more massive and heavy than those of any other matter, excepting gold. The greater or lesser fluidity, does not, therefore, indicate that the parts of the fluid are more or less weighty, but only that their adherence is so much the less, and their separation so much the easier.

Air, therefore, of all known matter, is that which heat divides the easiest, and is very near the nature of fire, whose property consists in the expansive motions of its parts; and it is from this similarity that air so strongly augments the activity of fire, to which it is the most powerful assistant, and the most intimate and necessary food. Even combustible matters will not keep it alive if deprived of air, for under this privation the most intense fire will not burn; but a single spark of air is sufficient to kindle them, and in proportion as it is supplied with that element the fire becomes strong, extended, and devouring.

Artificial phosphorus, and gunpowder, seem, at first, to be an exception, for they have no need of the assistance of renewed air to inflame and wholly consume them: their combustion may be performed in the closest vessels, but that is because those matters, which are also the most combustible, contain the necessary quantity of air in their substance, therefore they have no need of the assistance of foreign air.

This seems to indicate that the most essential difference between combustible matters and those which are not so, consists in the latter containing only a few or none of the light, ethereal, and oily matters susceptible of an expansive motion, or, at least, if they contain them, that they are fixed, so that they cannot exercise their volatility whenever the force of the fire is not strong enough to surmount the force of adhesion which retains them united to the fixed parts of matter. It may be said that this induction is confirmed by a number of observations well known to chemists; but what appears to be less so, and which, nevertheless, is a necessary consequence of it, is, that all matter may become volatile when the expansive force of the fire can be rendered superior to the attractive force which holds the parts of matter united; for though to produce a fire sufficiently strong it may require better constructed mirrors than any at present known, yet we are certain that fixity is only a relative quality, and that there is no matter absolutely so, since heat dilates the most fixed bodies. Now is not this dilation the index of a beginning separation, that may be augmented with a degree of heat to fusion, and with a still greater heat to volatilisation?

Combustion supposes something more than volatilisation; it is not sufficient that the parts of matter be sufficiently separated to be carried off by those of heat; they must also be of an analogous nature to fire; without that, mercury, being the most fluid next to air, would also be the most combustible, whereas experience demonstrates, that though very volatile it is not combustible. Matter is, in general, composed of four principal substances, called *elements*, that is, earth, water, air, and fire. Those in which earth and water predominate will be fixed, and will only become volatile by the action of heat; and those which contain most air and fire will be the only real combustibles. The great difficulty here is clearly to conceive how air and fire, both so volatile, can fix and become constituent parts of all bodies.

Fire, by absorbing air, destroys the spring. Now there are but two methods of destroying a spring, either by compressing it till it breaks, or extending it till it loses its effect. It is plain that fire cannot destroy air by compression, since the least degree of heat rarefies it; on the contrary, by a very strong heat the rarefaction of the air will be so great that it will occupy a space thirteen times more extended than that of its general volume; and by this means the spring becomes weakened, and it is in this state that it can become fixed, and unite with other bodies.

Light, which falls on bodies, is not merely reflected, but remains in quantities on the small thickness of the surface which it strikes; consequently it loses its motion, extends, is fixed, and becomes a constituent part of all that it penetrates. Let us add this light, transformed and fixed in bodies, to the above air, and to both, the constant and actual heat of the terrestrial globe, whose sum is much greater than that which comes from the sun, and then it will appear to be not only one of the greatest springs of the mechanism of Nature, but an element with which the whole matter of the globe is penetrated.

If we consider more particularly the nature of combustible matters, we shall find, that they all proceed originally from vegetables and animals; in a word, from bodies placed on the surface of the globe, which the sun enlightens, heats, and vivifies. Wood, bitumen, resins, coals, fat and oil, by expression, wax, and suet, are substances proceeding immediately from animals and vegetables. Turf, fossil, coal, amber, liquid, or concrete bitumens, are the productions of their mixture, and their decomposition,

position, whose ulterior waste forms sulphurs, and the combustible parts of iron, tin, pyrites, and every inflammable mineral. I know, that this last assertion will be rejected by those who have studied nature only by the mode of chemistry; but I must request them to consider, that their method is not that of nature, and that it cannot even approach it without banishing all those precarious principles, those fictitious beings which they play upon, without being acquainted with them.

But, without pressing longer on those general considerations, let us pursue in a more direct and particular manner the examination of fire and its effects. The action of fire depends much on the manner in which it is applied; and the effects of its motion, on similar subtances, will appear different according to the mode in which it is administered. I conceive that fire should be considered in three different states, first relative to its velocity; secondly, as to its volume; and thirdly, as to its mass. Under each of these points of view, this element, so simple, and so uniform to all appearance, will appear extremely different. The velocity of fire is augmented without the apparent volume being increased, every time that in a given space and filled with combustible matters, its action and expansion is pressed by augmenting the velocity of the air by bellows, caverns, ventilators, aspirative tubes, &c. all of which accelerate more or less the rapidity of the air directed on the fire. The action of fire is augmented by its volume, when a great quantity of combustible matters is accumulated, and the heat and fire are driven into the reverberatory furnaces, which comprehend those of our glass, porcelain, and pottery manufactories, and all those wherein metals and minerals are melted, iron excepted. Fire acts here by its volume, and has only its own velocity, since the rapidity is not augmented by the bellows, or other instruments which carry air to the fire.

There are many modes of augmenting the action of fire by its velocity or volume; but there is only one way of augmenting its mass; namely, by uniting it in the focus of a burning glass. When we receive on the refracting, or reflecting mirror, the rays of the sun, or even those of a well-kindled fire, we unite them in so much the less space, as the mirror is longer, and the focus shorter; for example, by a mirror of four feet diameter, and one inch focus, it is clear, that the quantity of light, or fire, which falls on the four-feet mirror, will be united in the space of one inch, that is, it will be 2304 times denser than it was, if all the incident matter arrived to this focus without any loss, and when even the loss is two thirds or three fourths, the mass of fire concentrated in the focus of this mirror, will always be six or seven hundred times denser than on the surface. In this, as in all other cases, the mass goes by the contraction of the volume; and the fire which we thus augment the density of, has all the properties of a mass of matter; for, independently of the action of heat, by which it penetrates bodies, it impels and displaces them as a solid moving body which strikes another would do.

Each of these modes of administering fire, and increasing either the velocity, volume, or mass, often produce very different effects on the same substances; insomuch, that no reliance is to be placed on any thing that cannot be worked at the same time, or successively, by all three. In the like manner, as I divide into three general proceedings the administration of this element, I divide every matter that can be submitted to its action into three classes. Passing over for the present those which are purely combustible, and which immediately proceed from animals and vegetables; we proceed to minerals, in the first class of which we reckon those mineral matters, which this action, continued for a long time, renders lighter, as iron; in the second, such as it renders heavier, as lead; and in the third class, are those matters on which, as gold, this action of fire does not appear to produce any sensible effect, since it does not at all alter their weight. All existing matters, that is, all substances simple and compounded, will necessarily be comprized under one of these three classes; and experiments on them by the three proceedings, which are not difficult to be made, and only require exactness and time, might develope many useful discoveries, and prove very necessary to build on real principles the theory of chemistry, which has hitherto been carried on by a precarious nomenclatura, and on words the more vague as they are the more general.

Fire is the lightest of all bodies, notwithstanding which it has weight, and it may be demonstrated, that even in a small volume it is really heavy, as it obeys, like all other matters, the general law of gravity, and consequently must have connections or affinities with other bodies. All matters it renders more weighty will be those with which it has the greatest affinity. One of the effects of this affinity in the matters is to retain the substance even of fire, with which it is incorporated, and this incorporation supposes that fire not only loses its heat and elasticity, but even all its motion, since it fixes itself in these bodies, and becomes a constituent part. From which it may be imagined that there is fire under a fixed and concrete form in almost every body.

It is evident, that all matters, whose weight increases by the action of fire, are endowed with an attractive force superior to the expansive, the fiery particles of which are animated; this being extinguished the motion ceases, and the elastic and fugitive particles become fixed, and take a concrete form. Thus matters, whose weight is increased by fire, as tin, lead, &c. are substances which, by their affinity with fire, attract and incorporate. All matters, on the contrary, which, like iron, copper, &c. become lighter in proportion as they are calcined, are substances whose attractive forces, relative to the igneous particles, is less than the expansive force of fire; and hence the fire, instead of fixing in these matters, carries off and drives away the least adherent parts which cannot resist its impulsion. Those which, like gold, platina, silver, &c. neither lose nor acquire by the application of fire, are substances which, having no affinity with fire, and not being able to unite, cannot, consequently, either retain or accompany it when it is carried off. It is evident that the matters of the two first classes have a certain degree of affinity with fire, since those of the second class are loaded with fire, which they retain; and the fire loads itself with those of the first class, which it carries off; whereas the matters of the third class, to which it neither lends nor borrows, have not any affinity or attraction with it, but are indifferent to its action, which can neither unnaturalize nor even change them.

This division of every matter into three classes, relative to the action of fire, does not exclude the more particular and less absolute division of all matters into two other classes, hitherto regarded as relative to their own nature, which is said to be always vitrifiable, or calcareous. Our new division is only a more elevated point of view, under which we must consider them, to endeavour to deduce therefrom even the agent that is used by the relations fire can have with every substance to which it is applied.

We might say, with naturalists, that all is vitrifiable in Nature, excepting that which is calcareous: that quartz, chrystals, precious stones, flints, granites, porphyries, agates, gypsums, clays, lava, pumice stone, with all metals and other minerals, are vitrifiable either by the fire of our furnaces, or that of mirrors; whereas marble, alabaster, stones, chalk, marl, and other substances which proceed from the residue of shells and madrepores, cannot be reduced into fusion by these means. Nevertheless I am persuaded, that if the power of our furnaces and mirrors were further increased, we should be enabled to put these calcareous matters in fusion; since there are a multiplicity of reasons to conclude, that at the bottom their substance is the same, and that glass is the common basis of all terrestrial matter.

By my own experiments I have found, that the most powerful glass furnaces is only a weak fire, compared with that of bellows furnaces; and that fire produced in the focus of a good mirror, is stronger than that of the most glowing fire of a furnace. I have kept iron ore for thirty-six hours in the hottest part of the glass furnace of Rouelle, in Burgundy, without its being melted, agglutinated, or even in any manner changed; whereas, in less than twelve hours this ore runs in a forge furnace. I have also melted, or volatilized, by a mirror many matters which neither the fire, nor reverberatory furnace, nor the most powerful bellows furnace could cause to run.

It is commonly supposed, that flame is the hottest part of fire, yet nothing is more erroneous than this opinion; the contrary may be demonstrated by the most simple and familiar experiments. Offer to a straw fire, or even to the flame of a lighted faggot, a cloth to dry or heat, and treble the time will be required to what would be necessary if presented to a brasier without flame. Newton very accurately defines flame to be a burning smoke, and this smoke, or vapour, has never the same quantity or intensity of heat as the combustible body from which it escapes. By being carried upwards and extending, it has the property of communicating fire, and carrying it further than the heat of the brasier, which alone might not be sufficient to communicate it when even very near.

The communication of fire merits a particular attention. I found, after repeated reflections that besides the assistance of facts which appear to have a relation to it, that experiments were necessary to understand the manner in which this operation of Nature is made. Let us recieve two or three thousand weight of iron in a mould at its issuing from the furnace; this metal in a short time loses its incandescence, and ceases from its redness, according to the thickness of the ingot. If at the moment its redness leaves it, it is drawn from the mold, the under parts will be still red, but this colour will fly off. Now so long as the redness subsists, we can light combustible matters by applying them to the ingot; but as soon as it has lost its incandescent state, there are numbers of matters which it will not set fire to, although the heat which it diffuses is, perhaps a hundred times stronger than that of a straw fire, which would inflame them. This made me think that flame being necessary to the communication of fire, there is therefore a flame in all incandescence. The red colour seems, in fact, to indicate it; and indeed I am convinced, that combustible, and even the most fixed matters, such as gold and silver, when in an incandescent state, are surrounded with a dense flame which extends only to a very short distance, and which is attached to their surface; and I can easily conceive, that when flame becomes dense to a certain degree, it ceases from obeying the fluctuation of the air. This white or red body, which issues from all bodies in incandescence, and which strikes our eyes, is the evaporation of this dense flame which surrounds the body by renewing itself incessantly on its surface; and even the light of the sun, which emits such an amazing brightness, I presume to be only an evaporation of the dense state that constantly plays on its surface; and which we must regard as a true flame, more pure and dense than any proceeding from our combustible matters.

It is, therefore, by light that fire communicates, and heat alone cannot produce the same effect as when it becomes very strong to be luminous. Even water, that destructive element to fire, by which alone we can prevent its progress, nevertheless communicates when in a well-closed vessel, such as Papin's digester, where it is penetrated with a sufficient quantity of fire to render it luminous, and capable of melting lead and tin, whereas when it is only boiling, far from communicating fire, it extinguishes it immediately. It is true, that heat alone is sufficient to prepare and dispose combustible bodies for inflammation, by driving off the humid parts from bodies; and what is very remarkable, this heat, which dilates all bodies, does not desist from hardening them by drying. I have an hundred times discovered, by examining the stones of my great furnaces, especially the calcareous, they increased in hardness in proportion to the time they had undergone the heat, and they also at the same time became specifically heavier. From this circumstance, I think an induction may be drawn, which would prove, and fully confirm, that heat, although in appearance always fugitive and never stable in the bodies which it penetrates, neverthelsss deposits in a positive manner many parts which fixes there even in greater quantities than the aqueous and other parts which it has driven off. But what appears very difficult to be reconciled, this same calcareous stone, which becomes specifically heavier by the action of a moderate heat a long time continued, becomes near a half lighter, when submitted to a fire sufficient for its calcination, and, at the same time, not only loses all the hardness it had acquired by the action of heat, but even the natural adherence of its constituting parts.

Calcination generally received, is, with respect to fixed and incombustible bodies, what combustion is to volatile and inflammable. Calcination, like combustion, needs the assistance of air; it operates so much the quicker, as it is furnished with a greater quantity of that element, without which the fiercest fire cannot calcine nor inflame any thing, except such matters as contain in themselves all the air necessary for those purposes. This necessity for the concurrence of air in calcination, as in combustion, indicates, that there are more things common between them than has been suspected. The application of fire is the principle of both; that of air is the second cause, and almost as necessary as the first; but these two causes are equally combined, according as they act in more or less time, and with more or less power on different substances.

Combustion operates almost instantaneously; calcination is sometimes so long, as to be thought impossible; for in proportion as matters are more incombustible, the calcination is there more slowly made; and when the constituent parts of a substance, such as gold, are not only incombustible, but appear so fixed as not to be volatilized, calcination produces no effect. They must both, therefore, be considered as effects of the same cause, whose two extremes are delineated to us by phosphorus, which is the most inflammable of all bodies, and by gold, which is the most fixed and least combustible. All substances comprized between these two extremes, will be more or less subjected to the effects of combustion and calcination, according as they approach either of them; insomuch, that in the middle points there will be found substances that endure an almost equal degree of both; from which we may conclude, that all calcination is always accopmanied with a little combustion, and all combustion with a little calcination. Cinders and other residue of the most combustible matters, demonstrate that fire has calcined all the parts it has not burned, and consequently, a little calcination is found here with combustion. The small flame which rises from most matters, that are calcined, demonstrates also that a slight combustion is made. Thus, we must not seperate these two effects, if we would find out the results of the action of fire on the different substances to which it is applied.

But it may be said, that combustion always diminishes the volume or mass, on account of the quantity of matter it consumes; and that, on the contrary, calcination increases the weight of many substances. Ought we then to consider these two effects whose results are so contrary, as effects of the same nature? Such an objection appears well-founded, and deserves an answer, especially as this is the most difficult point of the question. For that purpose let us consider a matter in which we shall suppose one half to be fixed parts, and the other volatile or combustible. By the application of fire to this, all the volatile or combustible parts will be raised up or burnt, and consequently separated from the whole mass; from hence this mass or quantity of matter will be found diminished one half, as we see it in calcareous stones, which lose near half their weight in the fire. But if we continue to apply the fire for a very long time to the other half, composed of fixed parts, all combustion and volatilization being ceased, that matter, instead of continuing to lose its mass, must increase at the expense of the air and fire with which it is penetrated; and those are matters already calcined, and prepared by Nature to the degree where combustion ceases, and consequently susceptible of increasing the weight from the first moment of the application. We have seen, that light extinguishes on the surface of all bodies which do not reflect; and that heat, by long residence, fixes partly in the matters which it penetrates; we know also that air is necessary for calcination, or combustion, and the more so for calcination as having more fixity in the external parts of bodies, and becomes a constituent part: hence, it is natural to imagine, that this augmentation of weight proceeds only from the addition of the particles of light, heat, and air, which are at length fixed and united to one matter, against which they have made so many efforts, without being able either to raise or burn them. This appears clearly to be the fact, for if we afterwards present a combustible substance to them they will quit the fixed matter, to which they were only attached through force, retake their natural motion, elasticity, and volatility, and all depart with it; from hence, metal, or calcinized matter, to which these volatile parts has been rendered, retakes its pristine form, and its weight is found diminished by the whole quantity of fiery and airy particles which were fixed in it, and which had been just raised by this new combustion. All this is performed by the sole law of affinities; and their seems to be no more difficulty to conceive how the lime of a metal is reduced, than to understand how it is precipitated in dissolution; the cause is the same, and the effects are similar. A metal dissolved by an acid, will precipitate when to this acid another substance is offered with which it has more affinity than metal, the acid then quits it and falls to the bottom. So, likewese, this metal calcines, that is, loaded with parts of air, heat, and fire, which being fixed, keeps it under the form of a lime, and will precipitate, or be reduced, when presented to this fire and fixed air, from the combustible matters with which they have more affinity than with the metal; the latter will retake its first form as soon as it is disembarrassed from this superfluous air and fire, at the expence of the combustible matters offered to it, and the volatile parts it had lost.

I think I have now demonstrated, that all the little laws of chemical affinities, which appeared so variable and different, are no other than the general laws of attraction, common to all matter; that this great law, always constant and the same, appeared only to vary in its expression, which cannot be the same when the figure of bodies enters, like an element, into their distance. With this new key we can unlock the most profound secrets of Nature; we can attain the knowledge of the figure of the primitive parts of different substances; assign the laws and degrees of their affinities; determine the forms which they take by re-uniting, &c. I think also I have made it appear that impulsion depends on attraction; and that, although it may be considered as a different force, it is, notwithstanding, a particular effect of this sole and general one. I have shewn the communication of motion to be impossible without a spring, whence I have concluded, that all bodies in Nature are more or less elastic, and that there is not one perfectly hard; that is, entirely deprived of a spring, since all are susceptible of receiving motion. I have endeavoured to shew how this sole force may change direction, and attraction become repulsion; and from these grand principles, which are all founded on rational mechanics, I have sought to deduce the principal operations of Nature, such as the production of light, heat, and fire, and their action on different substances; this last object which interests us the most is a vast field, but of which I can only cultivate a little spot, yet I presume I may render some assistance, by putting into more capable and laborious hands the instruments I made use of. These instruments were the three modes of making use of fire, that is, by its velocity, volume, and mass; by applying it concurrently to the three classes of substances, which either lose, gain, or are not affected by the application of fire. The experiments which I had made on the refrigeration of bodies, on the real weight of fire, on the nature of flame, on the progress of heat, or its communication, its diperdition, its concentration, or its violent action without flame, &c. are also so many instruments which will spare much labour to those who choose to avail themselves of them, and will produce an ample harvest of knowledge.

OF AIR, WATER, AND EARTH.

BY our former observations it appears that air is the necessary and first food of fire, which can neither subsist nor propagate but by what it assimilates, consumes, or carries off, of that element, whereas of all material substances, air is that which seems to exist the most independently of the aid or presence of fire; for although it habitually has nearly the same heat as other matters on the surface of the earth, it can do without it and requires infinitely less than any of the rest to support its fluidity, since the most excessive cold cannot deprive it of that. The strongest condensations are not capable of breaking its spring; the active fire, in combustible matters, is the only agent which can alter its nature by rarefying and extending its spring to the point of rendering it ineffectual, and thus destroying its elasticity. In this state, and in all the links which precede, the air is capable of re-assuming its elasticity, in proportion as the vapours of combustible matters evaporate and separate from it. But if the spring have been totally weakened and extended that it cannot re-instate itself, from having lost all its elastic power, the air, volatile as it might before have been, becomes a fixed substance which incorporates with the other substances, and forms a constituent part of all those to which it unites by contact. Under this new form it can no longer forsake the fire, except to unite, like fixed matter, to other fixed matters; and if there remain some parts inseparable from fire, they then make a portion of that element serve it for a base, and are deposited with it in the substance they heat and penetrate together. This effect is manifested in all calcinations, and is the more sensible as the heat is longer; but combustion demands only a small time to completely effectuate the same. If we wish to hasten calcination the use of bellows may be necessary, not so much to augment the heat of the fire as to establish a current of air on the surface of the matters; yet it is not requisite for the fire to be very fierce to deprive air of its elasticity, for a very moderate heat, when constantly applied on a small quantity, is sufficient to destroy the spring; and for this air, without spring, to fix itself afterwards in bodies, there is only a little more or less time required, according to the affinity it may have under this new form, with the matters to which it unites. The heat of the body of animals, and even vegetables, is sufficiently powerful to produce this effect. The degrees of heat are different in different kinds of animals: birds are the hottest, from which we pass successively to quadrupeds, man, cetaceous animals, reptiles, fish, insects, and, lastly, to vegetables, whose heat is so trifling as to have made some naturalists declare they had not any, although it is very apparent, and in winter surpasses that of the atmosphere. I have frequently observed in trees that were cut in cold weather, that their internal part was sensibly warm, and that this heat remained for many minutes. This heat is only moderate while the tree is young and sound, but as soon as it grows old the heart heats by the fermentation of the pith, which no longer circulates there with the same freedom; and as soon as this heat begins the centre receives a red tint, which is the first index of the perishing state of the tree, and the disorganization of the wood. The reason naturalists have not found there was a difference between the temperature of the air, and the heat of vegetables is, because they have made their observations at a bad time of the year, and not paid attention, that in the summer the heat of the air exceeds that of the internal part of a tree; whereas in winter it is quite the contrary. They have not remembered that the roots have constantly the degree of heat which surrounds them, and that this heat of the internal part of the earth is, during all winter, considerably greater than that of the air, and the surface of the earth. They did not consider that the motion alone of the pith, already warm, is a necessary cause of heat, and that this motion, increasing by the action of the sun, or by an external heat, that of vegetables must be so much the greater as the motion of their pith is more accelerated, &c.

Here the air contributes to the animal and vital heat, as we have seen that it does to the action of fire in combustible and calcinable matters. Animals, which have lungs, and which consequently respire the air, have more heat than those deprived of them; and the more the internal surface of the lungs is extended, and ramified in a greater number of cells, the more it presents greater superficies to the air which the animal draws by inspiration; the more also its blood becomes hotter, the more it communicates heat to all parts of the body it nourishes, and this proportion takes place in all known animals.

Birds, relatively to the volume of their body, have lungs considerably more extended than man or quadrupeds. Reptiles, even those with a voice, as frogs, instead of lungs have a simple bladder. Insects which have little or no blood breathe the air only by some pipes, &c. Thus taking the degree of the temperature of the earth for the term of comparison, I have observed that this heat being supposed ten degrees, that of birds was nearly thirty-three, that of some quadrupeds more than thirty-one and a half, that of man thirty and a half, or thirty-one, whereas that of frogs is only fifteen or sixteen, and that of fishes and insects only eleven or twelve, which is nearly the same as that of vegetables. Thus the degree of heat in man and animals depends on the force and extent of the lungs; these are the bellows of the animal machine: the only difficulty is to conceive how they carry the air on the fire which animates us, a fire whose focus seems to be indeterminate; a fire that has not even been qualified with this name, because it is without flame or any apparent smoke, and its heat is only moderate and uniform. However, if we consider that heat and fire are effects, and even elements of the same class; that heat rarefies air, and, by extending its spring, it may render it without effect; we may imagine, that the air drawn by our lungs being greatly rarefied, loses its spring in the bronchiæ and little vesicles, where it is soon destroyed by the arterial and venous blood, for these blood-vessels are separated from the pulmonary vesicles by such thin divisions that the air easily passes into the blood, where it produces the same effect as upon common fire, because the heat of this blood is more than sufficient to destroy the elasticity of the particles of air, and to drag them under this new form into all the roads of circulation. The fire of the animal body differs from common fire only in more or less; the degree of heat is less, hence there is no flame, because the vapours, which represent the smoke, have not heat enough to inflame; every other effect is the same: the respiration of a young animal absorbs as much air as the light of a candle, for if inclosed in vessels of equal capacities, the animal dies in the same time as the candle extinguishes: nothing can more evidently demonstrate that the fire of the animal and that of the candle are not of the same class but of the same nature, and to which the assistance of the air is equally necessary.

Vegetables, and most insects, instead of lungs, have only aspiratory tubes, by which they pump up the air that is necessary for them; it passes in very sensible balls into the pith of the vine. This air is not only pumped up by the roots but often even by the leaves, and forms a very essential part of the food of the vegetable which assimilates, fixes, and preserves it. Experience fully confirms all we have advanced on this subject, and that all combustible matters contain a considerable quantity of fixed air, as do also all animals and vegetables, and all their parts, and the waste which proceeds therefrom; and that the greatest number likewise include a certain quantity of elastic air. And, notwithstanding the chimerical ideas of some chemists, respecting phlogiston, there does not remain the smallest doubt but that fire or light produces, with the assistance of air, all the effects thereof.

Minerals, which like sulphur and pyrites, contain in their substance a quantity of the ulterior waste of animals and vegetables, contain thence combustible matters, which, like all other, contain more or less fixed air, but always much less than the purely animal or vegetable substances. This fixed air can be equally removed by combustion. In animal and vegetable matters it is disengaged by simple fermentation, which, like combustion, has always need of air for its operation. Sulphurs and pyrites are not the only minerals which must be looked upon as combustible, there are many others which I shall not here enumerate, because it is sufficient to remark, their degree of combustion depends commonly on the quantity of sulphur which they contain. All combustible minerals originally derive this property either from the mixture of animal or vegetable parts which are incorporated with them, or from the particles of light, heat, and air, which, by the lapse of time, are fixed in their internal part. Nothing, according to my opinion, is combustible but that which has been formed by a gentle heat, that is, by these same elements combined in all the substances which the sun brightens and vivifies, or in that which the internal heat of the earth foments and unites.

The internal heat of the globe of the earth must be regarded as the true elementary fire; it is always subsisting and constant; it enters, like an element, into all the combinations of the other elements, and is more than sufficient to produce the same effects on air as actual fire on animal heat; consequently this internal heat of the earth will destroy the elasticity of the air, and render it fixed, which being divided into minute parts will enter into a great number of substances, from hence they will contain articles of fixed air and fire, which are the first principles of combustibility; but they will be found in different quantities, according to their degree of affinity with the substance, and this degree will greatly depend on the quantity these substances contain of animal and vegetable parts, which appear to be the base of all combustible matter. Most metallic minerals, and even metals, contain great quantities of combustible parts; zinc, antimony, iron, copper, &c. burn and produce a very brisk flame, as long as the combustion of these inflammable parts remains, after which, if the fire be continued, the calcination begins, during which there enters into them new parts of air and heat, which fixes, and cannot be disengaged but by presenting to them combustible matters, with which they have a greater affinity than with those of the mineral, with which they are only united by the effort of calcination. It appears to me, that the conversion of metallic substances into dross, and their reproduction, might be very clearly understood without applying to secondary principles, or arbitrary hypotheses, for their explanation.

Having considered the action of fixed air in the most secret operations of nature, let us take a view a view of it when it resides in bodies under an elastic form; its effects are then as variable as the degrees of its elasticity, and its action, though always the same, seems to give different products in different substances. To bring this consideration back to a general point of view, we will compare it with water and earth, as we have already compared it with fire; the results of this comparison between the four elements will afterwards be easily applied to every substance, since they are all composed merely of these four real principles.

The greatest cold that is known, cannot destroy the spring of the air, and the least heat is sufficient for that purpose, especially when this fluid is divided into very small particles. But it must be observed, that between its state of fixity, and that of perfect elasticity, there are all the links of the intermediate states, in one of which it always resides in earth and water, and all the substances which are composed of them; for example, water, which appears so simple a substance, contains a certain quantity of air, which is neither fixed nor elastic, as is plain from its congulation, ebullition, and resistence to all compression, &c. Experimental philosophy demonstrates, that water is incompressible, for instead of shrinking and entering into itself when pressed, it passes through the most solid and thickest vessels; which could not be the case if the air it contained were in a state of full elasticity. The air contained therefore in water, is not simply mixed therewith, but is united in a state where its spring is not sensibly exercised; yet the spring is not entirely destroyed, for if we expose water to congelation, the air issues from its internal part, and unites on its surface in elastic bubbles. This alone suffices to prove, that air is not contained in water under its common form, since being specifically 850 times lighter, it would be forced to issue out by the sole necessity of the preponderance of water; neither under an affixed form, but only in a medium state, from whence it can easily retake its spring, and separate more easily than from every other matter.

It may, with some justice, be objected that cold and heat never operate in the same mode, and that if one of these causes gives to air its elasticity, the other must destroy it, and I own that in general it is so, but in this particular they produce the same effect. It is well known that water, frozen or boiled, reabsorbs the air it had lost as soon as it is liquefied or cooled. The degree of affinity of air with water, depends, therefore, in a great measure, on its temperature, which in its liquid state, is nearly the same as that of the general heat, to the surface of the earth: the air with which it has much affinity penetrates it as soon as it is divided into small parts, yet the degree of elementary and general heat, weakens their spring so as to render them ineffectual as long as the water preserves this temperature; but if the cold penetrate, or this degree of heat diminish, then its spring will be re-established by the cold, and the elastic bubbles will rise to the surface of the water ready to freeze; if, on the contrary, the temperature of the water is increased by an external heat, the integrant parts become too much divided, they are rendered volatile, and the air with which they are united, rises and escapes with them. Water and air have much greater connections between them than opposite properties, and as I am well persuaded, that all matter is convertible, and that the elements may be transformed, I am inclined to believe, that water can change into air when sufficiently rarefied to raise up in vapours, for the spring of the vapour of the water is even more powerful than the spring of the air.

Experience has taught me that the vapours of water can increase the fire in the same manner as common air; and this air, which we may regard as pure, is always mixed with a very a very great quantity of water; but it must be remarked, as an observation of much importance, that the proportions of the mixtures are not nearly the same in these two elements. It may be said in general that there is much less air in water than water in air. In considering this proportion we must refer to the volume and mass. If we estimate the quantity of air contained in water by the volume it will appear nil, since the volume is not in the least increased. Thus it is not to the volume that we must relate this proportion, it is alone to the mass, that is, to the real quantity of matter in one and the other of these two elements that we must compare that of their mixture, by which we shall perceive that the air is much more *aqueous* than the water is *aerial*, perhaps in proportion of the mass, that is, eight hundred and fifty times. Be this estimation either too strong or too weak we can derive this induction from it, that water must change more easily into air than air can transform into water. The parts of air, although susceptible of being extremely divided, appear to be more gross than those of water, since the latter passes through many filtres which air cannot penetrate; since the vapours of water are only raised to a certain height in the air; and, in short, since air seems to imbibe water like a sponge, to contain it in a large quantiy; and that the container is certainly greater than the contained.

In the order of the conversion of the elements it appears to me, that water is to air what air is to fire, and that all the transformations of nature depend on them. Air, like the food of fire, assimilates with it, and is transformed into this first element. Water, rarefied by heat, is transformed into a kind of air capable of feeding the fire like common air. Thus fire has a double fund of certain subsistence; if it consume much air it can also produce much by the rarefaction of water, and thus repair, in the mass of atmosphere, all the quantity it destroyed, while ulteriorly it converts itself with air into fixed matter in the terrestrial substances which it penetrates by its heat or by its light. And so, likewise, as water is converted into air, or into vapours, as volatile as air, by its rarefaction, it is also converted into a solid substance by a kind of condensation. Every fluid is rarefied by heat and condensed by cold. Water follows this common law, and condenses as it grows cold. Let a glass tube be filled three parts full and it will descend in proportion as the cold increases, but some time before congelation it will ascend above the point of three fourths of the height of the tube, and increase still more considerably by being frozen. But if the tube be well stopped, and perfectly at rest, the water will continue to descend, and will not freeze, although the degree of cold be six, eight, or ten degrees below the freezing point; congelation, therefore, presents, in an inverted manner, the same phenomena as inflammation. A heat, however great, shut up in a well-closed vessel, will not produce inflammation unless touched with an inflamed matter; so, likewise, to whatsoever degree a fluid is cooled, it will not freeze unless it touch something already frozen, and this is what happens when the tube is shaken or uncorked; the particles of water, which are frozen in the external air, or in the air contained in the tube, strike the surface of the water, and communicate their ice to it. In inflammation, the air, at first very much rarefied by heat, loses its volume, and fixes itself suddenly. In congelation, water, at first condensed by the cold, takes a larger volume, and fixes itself likewise, for ice is a solid substance, lighter than water, and would preserve its solidity if the cold continued the same; and I am inclined to believe that we may attain the point of fixing mercury at a less degree of cold, by sublimating it into vapours in a very cold air; and also that water, which only owes its liquidity to heat, would become a substance much more solid and fusible, as it would endure a stronger and a longer time the rigour of the cold.

But without stopping upon this subject, that is, without admitting or excluding the possibility of the conversion of the ice into infusible matter, or fixed and solid earth, let us pass on to more extensive views on the modes which Nature makes use of for the transformation of water. The most powerful of all and the most evident is the animal filter. The body of shell-animals, by feeding on the particles of water, labours, at the same time, on the substance to the point of unnaturalizing it. The shell is certainly a terrestrial substance, a true stone, from which all the stones called *calcareous*, and many other matters, derive their origin. This shell appears to make the constitutive part of the animal it covers, since it is perpetuated by generation, for it is on the small shell-animal just come into existence as well as on those which have arrived at their full growth; but this is no less a terrestrial substance, formed by the secretion or exudation of the body, for it increases and thickens by rings and layers in proportion as the animal grows; and stony matter often exceeds fifty or sixty times the mass of the body which produces it. Let us, for a moment, reflect on the number of the kind of shell-animals, or rather of those animals with a stony transudation; they, possibly, are more numerous in the sea than the insect kind are upon earth. Let us afterwards represent their full growth, their prodigious multiplication, and the shortness of their lives, which we may suppose does not exceed ten years; let us then consider that we must multiply by fifty or sixty the almost immense number of the individuals of this class to form an idea of all the stony matter produced in ten years; then that this block must be augmented with as many similar blocks as there are as many times ten in all the ages from the beginning of the world, and by this means we shall conceive, that all our córal, rocks of calcareous stone, marble, chalk, &c. originally proceeded alone from the cast-off coats of those little animals.

Salts, bitumen, oil, and the grease of the sea, enter little or none into the composition of the shell; neither does the calcareous stone contain any of those matters; this stone is, therefore, only water transformed, joined to some little portion of vitrifiable earth, and to a great quantity of fixed air, which may be disengaged by calcination. This operation produces the same effect on the shells taken in the sea as upon those drawn out of quarries; they both form lime, with only a little difference in their quality. Lime, made with oyster or other shells, is weaker than that made with marble or hard stone; but the process of Nature is the same, as are the results of its operation. Both shells and stones, lose nearly half their weight by the action of fire in calcination; the water issues first, after which the fixed air is disengaged, and then the fixed water, of which these stony substances are composed, resumes its primitive nature, is elevated into vapours, drove off and rarefied by the fire, so that there remains only the most fixed parts of this air and water, which, perhaps, are so strongly united in themselves, and to the small quantity of the fixed earth of the stone, that the fire cannot separate them; the mass, therefore, is reduced nearly a half, and would probably be still more if submitted to a stronger fire. And what appears to me to prove that this matter, driven out of the stone by the fire, is nothing else than air and water, is the avidity with which calcined stone sucks up the water given to it, and the force with which it draws water from the atmosphere. Lime, by exposure either in air or water, in a great measure regains the mass it had lost by calcination; the water, with the air it contains, replaces that which the stone contained before. Stone then retakes its first nature, for in mixing lime with the remains of other stones, a mortar is made which hardens, and becomes a solid substance, like those from which it is composed.

Thus, then, we see on the one hand all the calcareous matters, the origin of which we must refer to animals; and on the other, all the combustible matters proceeding from animal or vegetable substances; they occupy together a great space on the earth; yet, however great their number may be, they only form a small part of the terrestrial globe, the principal foundation of which, and the greatest quantity consists in one matter of the nature of glass; a matter we must look upon as terrestrial element, to the exclusion of all other substances, to which it serves as a base, like earth, when it forms vegetables by the means, or remains of animals, and by the transformation of the other elements; and it is also the ulterior term to which we can return or reduce them all.

It appears that the animal filter converts water into stone; the vegetable filtre can also transform it, when all the circumstances are found to be the same. The heat of vegetables and the organs of life being less powerful than those of shell animals, the vegetables can produce only a small quantity of stones, which are frequently found in its fruits ; but it can and does convert a great quantity of air, and a still greater of water into its substance. It may be asserted, without fear of contradiction, that the fixed earth it appropriates, and which serves as a base to these two elements, does not make the hundredth part of its mass; hence, the vegetable is almost entirely composed of air and water, transformed into wood, or a solid substance, which is afterwards reduced into earth by combustion and putrefaction. The same may be said of animals; they not only fix and transform air and water, but fire, and in a much greater quantity than vegetables. It appears, therefore, to me, that the functions of organized bodies are the most powerful means made use of by Nature for the conversion of the elements. We may regard each animal, or vegetable, as a small particular centre of heat or fire that appropriates to itself the air and water which surround it, assimilates them to vegetate or nourish, and live on the productions of the earth, which are themselves only air and water previously fixed. It also appropriates to itself a small quantity of earth, and receiving the impressions of light, the heat of the sun and terrestrial globe, it converts into its substance all these different elements; works, combines, unites, and opposes them, till they have undergone the necessary form towards its support of life, and the growth of organization, the mold of which once given, models every matter it admits, and from inanimate renders it organized.

Water, which so readily coalesces and enters with air into organized bodies, unites also with some solid matters, such as salts; and it is often by their means that it enters into the composition of minerals. Salt at first appears to be only an earth soluble in water, and of a sharp flavour, but chemists have perfectly discovered, that it principally consists in the union of what they term the *earthly* and the *aqueous principle*. The experiment of the nitrous acid, which after combustion leaves only a small quantity of earth and water, has caused them to think, that salt was composed only of these two elements; yet I think it is easily to be demonstrated, that air and fire also enter their composition; since nitre produces a great quantity of air in combustion, and this fixed air supposes fixed fire which disengages at the same time: besides all the explanations given of the dissolution cannot be supported,

ported, and it would be against all analogy, that salt should be composed only of these two elements, while all other substances are composed of four. Hence we must not receive literally what those great chemists Messrs. Stahl and Macquer have said on this subject; the experiments of Mr. Hales demonstrate, that vitriol and marine salt contain much fixed air; that nitre contains still more, even to the eighth of its weight; and that salt of tartar contains still more than these. It may, therefore, be asserted that air enters as a principle into the composition of all salts; but this does not support the idea that salt is the mediate substance between earth and water; these two elements enter in different proportions into the different salts or saline substances, whose variety and number are so great, as not to be enumerated; but which, generally presented under the denomination of acids and alkalis, shews us, that there is in general more earth than water in the last, and more water than earth in the first.

Nevertheless, water, although it may be intimately mixed with salts, is neither fixed nor united there by a sufficient force to transform it into a solid matter like calcareous stone; it resides in salt or acid under its primitive form, and the best concentrated acid, or the most deprived of water, which might be looked upon as liquid earth, only owes its liquidity to the quantity of the air and fire it contains; and it is no less certain, that they are indebted for their savour to the same principles. An experiment which I have frequently tried, has fully convinced me, that alkali is produced by fire. Lime made according to the common mode, and put upon the tongue, even before slacked by air or water, has a savour which indicates the presence of a certain quantity of alkali. If the fire be continued, this lime by longer calcination, becomes more poignant; and that drawn from furnaces, where the calcination has subsisted for five or six months together, is still more so. Now this salt was not contained in the stone before its calcination; it augmented in proportion to the strength and continuance of the fire; it is therefore evident, that it is the immediate product of the fire and air, which incorporate in the substance during its calcination, and which, by this means, are become fixed parts of it, and from which they have driven most of the watry molecules it before contained. This alone appeared to me sufficient to pronounce that fire is the principal of the formation of the mineral alkali; and we may conclude, by analogy, that other alkalis owe their formation to the constant heat of the animal and vegetable from which they are drawn.

With respect to acids, although the demonstration of their formation by fire and fixed air, is not so immediate as that of alkalis, yet it does not appear less certain. We have proved, that nitre and phosphorus draw their origin from vegetable and animal matters: that vitriol comes from pyrites, sulphur and other combustibles. It is likewise certain that acids, whether vitriolic, nitrous, or phosphoric, always contain a certain quantity of alkali; we must, therefore, refer their formation and savour to the same principle, and by reducing the varieties of both to one of each, bring back all salts to one common origin: those which contain most of the active principles of air and fire, will necessarily have the most power and taste. I understand by power the force with which salts appear animated to dissolve other substances. Dissolution supposes fluidity, and as it never operates between two dry or solid matters, it also supposes the principle of fluidity in the dissolvent, that is, fire; the power of the dissolvent will be, therefore, so much the greater, as on one part it contains more of this active principle; and, on the other hand, its aqueous and terrene parts will have more affinity with those of the same kind contained in the substances to dissolve; and, as the degrees of affinity vary, we must not be surprized at different salts varying in their action on different substances; their active principle is the same, their dissolving power the same; but they remain without exercise when the substance presented repels that of the dissolvent, or has no degree of affinity with it; but the contrary is the case when there is sufficient force of affinity to conquer that of the coherence; that is, when the active principles, contained in the dissolvent, under the form of air and fire, are found more powerfully attracted by the substance to be dissolved, than they are by the earth and water they contain. Newton is the first who has assigned affinities as the causes of chemical precipitation; Stahl adopted this idea and transmitted it to all the other chemists; and it appears to be at present universally received as a truth. But neither Newton nor Stahl saw that all these affinities, so different in appearance, are only particular effects of the general force of universal attraction: and, for want of this knowledge, their theory cannot be either luminous or complete, because they were obliged to suppose as many trivial laws of different affinities, as there were different phenomena; instead of which there is in fact only one law of affinity, a law which is precisely the same as that of universal attraction.

Salts concur in many operations of Nature by the power they have of dissolving other substances; for, although it is commonly said, that water dissolves salt, it is easy to be perceived, that in reality, when there is a dissolution, both are active, and may be alike called *dissolvents*. Regarding salt as only a dissolvent, the body to be dissolved may be either liquid or solid; and, provided the parts of the salt be sufficiently divided to touch immediately those of the other substances, they will act and produce all the effects of dissolution. By this we see how much the action of salts, and the action of the element of water which contains them, must have influence on the composition of mineral matters. Nature may produce by this mode, all that our arts produce by that of fire. Time only is required for salts and water to produce on the most compact and hard substances, the most complete division and attenuation of their parts, so as to render them capable of uniting with all analagous substances, and to separate from all others; but this time, which to Nature is never wanting, is, of all things, that which is the most deficient to us: the greatest of all our arts, therefore, is that of abridging time, that is, to effect that in one day, which nature takes an age to perform. However vain this pretension may appear, we must not entirely renounce it, for has not man discovered the mode of creating fire, of applying it to his use, and by the means of this element to suddenly dissolve those bodies by fusion which would require a considerable period by any other means?

We must not, however, conclude that Nature really performs by the means of water all that we do by fire. The decomposition of every substance is only to be made by division, and the greater this division the more the decomposition will be complete. Fire seems to divide as much as possible those matters which it fuses; nevertheless it may be doubted whether those which water and acids keep in dissolution are not still more divided, and the vapours raised by heat contain matters still further attenuated, in the bowels of the earth, then, by the means of the heat it includes, and the water which insinuates, there is made an infinity of sublimations; distillations, chrystallizations, aggregations, and disjunctions, of every kind. By time all substances may be compounded and decompounded by these means; water may divide and attenuate the parts more than fire when it melts them, and those attenuated parts will join in the same manner as those of fused metal unite by cooling. Crystallization, of which the salts have given us an idea, is never performed but when a substance, being disengaged from every other, is much divided and sustained by a fluid, which having little or no affinity with it, permits it to unite and form by virtue of its force of attraction, masses of a figure nearly similar to its primitive parts. This operation, which supposes all the above circumstances, may be done by the intermediate aid of fire as well as by that of water, and is often accomplished by the concurrence of both, because all this exacts but one division of matter sufficiently great for its primitive parts to be able to form, by uniting figured bodies like themselves. Now fire can bring many substances to this state much better than any other dissolvent, as observation demonstrates to us in asbestos, and other productions of fire, whose figures are regular, and which must be looked upon as true crystallizations. Yet this degree of division, necessary to crystallization, is not the greatest possible, since in this state the small parts of matter are still sufficiently large to constitute a mass, which like other masses, is only obedient to the sole attractive force, and the volumes of which, only touching in points, cannot acquire the resultive force that a much greater division might perform by a more immediate contact, and this is what we see happen in effervescences, where at once, heat and light are produced by the mixture of two cold liquors.

Light, heat, fire, air, water, and salts, are steps by which we descend from the top of Nature's ladder to its base, which is fixed earth. And these are at the same time the only principles that we must admit and combine for the explanation of all phenomena. These principles are real, independently of all hypotheses and all method, as are also their conversion and transformation, which are demonstrated by experience. It is the same with the element of earth, it can convert itself by volatilizing and taking the form of the other elements, as those take that of earth in fixing; it, therefore, appears quite useless to seek for a substance of pure earth in terrestrial matters. The transparent lustre of the diamond dazzled the sight of our chemists, when they considered that stone as a pure elementary fire; they might have said with as much foundation, that it is pure water, all the parts of which are fixed to compose a solid diaphanous substance. When we would define Nature, the large masses should alone be considered, and those elements have been well taken notice of by even the most ancient philosophers.

sophers. The sun, atmosphere, earth, sea, &c. are all great masses on which they established all their conclusions; and if there ever had existed a planet of phlogiston, an atmosphere of alkali, an ocean of acid, or a mountain of diamonds, such might have been looked upon as the general and real principles of all bodies, but they are only particular substances, produced, like all the rest, by the combinations of true elements; and ideas to the contrary would never have been started but upon the supposition that the earth was neither more simple nor less convertible than either of the other elements.

In the great mass of solid matter, which the earth represents, the superficial is the least pure. All the matter deposited by the sea, in form of sediment, all stones produced by shell-animals, all substances composed by the combinations of the waste of the animal or vegetable kingdom, and all those which have been changed by the fires of volcanos, or sublimated by the internal heat of the globe, are mixed and transformed substances; and although they compose great masses they do not clearly represent to us the element of earth. They are vitrifiable matters, whose mass must be considered as 100,000 times more considerable than all those other substances, which should be regarded as the true basis of this element.

It is from this common foundation that all other substances have derived the origin of their solidity, for all fixed matter, however much decomposed, subsides finally into glass by the sole action of fire: it resumes its first nature, when disengaged from the fluid, or volatile matters, which were united with it; and this glass, or virtreous matter, which composes the mass of our globe, represents so much the better the element of earth, as it has neither colour, smell, taste, liquidity, nor fluidity, qualities which all proceed from the other elements, or belong to them.

If glass be not precisely the element of earth, it is at least the most ancient substance of it; metals are more recent, and less dignified; and most other minerals form within our sight. Nature produces glass only in the particular focus of its volcanos, whereas every day she forms other substances by the combination of glass with the other elements. If we would form to ourselves a just idea of her formation of the globe, we must first consider her processes, which demonstrate that it has been melted or liquefied by fire; that from this immense heat it successively passed to its present degree; that in the first moments, where its surface began to take consistence, inequalities must be formed, such as we see on the surface of melted matters grown cold: that the highest mountains, all composed of vitrifiable matters, existed and take their date from that moment, which is also that of the seperation of the great masses of air, water, and earth; that afterwards, during the long space of time which the diminution of the heat of the globe to the point of present temperature supposes, there were made in these mountains, which were the parts most exposed to the action of external causes, an infinity of fusions, sublimations, aggregations, and transformations, by the fire of the sun, and all the other causes which this great heat rendered more active than they at present are, and that consequently we must refer back to this date the formation of metals and minerals which we find in great masses, and in thick and continued veins. The violent fire of inflamed earth, after having raised up and reduced into vapours all that was volatile, after having driven off from its internal parts the matters which compose the atmosphere and the sea, and at the same time sublimated all the least fixed parts of the earth, raised them up and deposited them in every void space, in all the cavities which formed on the surface in proportion as it cooled; this, then, is the origin and the gradation of the situation and formation of vitrifiable matters which fire has divided, formed and sublimated

After this first establishment (and which still subsists) of vitrifiable matters and minerals into a great mass, which can be attributed to the action of fire alone, water which till then formed with air only a vast volume of vapours, began to take its present state; it collected and covered the greatest part of the surface of the earth, on which, finding itself agitated by a continual flux and reflux, by the action of winds and heat, it began to act on the works of fire: it changed, by degrees, the superficies of vitrifiable matters; it transported the wrecks and deposited them in the form of sediments; it nourished shell-animals,it collected their shells, produced calcareous stones, formed hills and mountains, which becoming afterwards dry, received in their cavities all the mineral matters they could dissolve or contain.

To establish a general theory on the formation of Minerals, we must begin then by distinguishing with the greatest attention,first, those which have been produced by the primitive fire of the earth while it was burning with heat; secondly, those which have been formed from the waste of the first by the means of water; and thirdly, those which in volcanos,

canos, or other subsequent conflagrations, have a second time undergone the proof of a violent heat. These three objects are very distinct, and comprehend all the mineral kingdom; by not losing sight of them, and by connecting each substance, we can scarcely be deceived in its origin, or even in the degrees of its formation. All minerals which are found in masses, or large veins in our high mountains, must be referred to the sublimation of the primitive fire; all those which are found in small ramifactions, in threads or in vegetations, have been formed only from the waste of the first hurried away by the stillation of waters. We are evidently convinced of this, by comparing the matter of the iron mines of Sweden with that of our own. These are the immediate work of water, and we see them formed before our eyes; they are not attracted by the load stone; they do not contain any sulphur, and are found only dispersed in the earth; the rest are all more or less sulphureous, all attracted by the load stone, which alone supposes that they have undergone the action of fire; they are disposed in great, hard, and solid masses: and their substance is mixed with a quantity of asbestos, another index of the action of fire. It is the same with other metals: their ancient foundation comes from fire, and all their great masses have been united by its action; but all their crystallizations, vegetations, granulations, &c. are due to the secondary causes, in which water is the primary agent.

EXPERIMENTS ON THE PROGRESS OF HEAT IN MINERAL SUBSTANCES.

I CAUSED ten bullets to be made of forged and beaten iron; the first, of half-inch diameter; the second, of an inch; and so on progressively to five inches: and as all the bullets were made of iron of the same forge, their weights were found nearly proportionable to their volumes.

The bullet of half an inch weighed 190 grains, Paris weight; that of an inch, 1522 grains; that of an inch and a half, 5136 grains; that of two inches, 12173 grains; that of two inches and an half, 23781 grains; that of three inches, 41085 grains; that of three inches and a half, 65254 grains; that of four inches, 97388 grains; that of four inches and an half, 138179 grains; and that of five inches, 190211 grains. All these weights were taken with very good scales, and those bullets which were found too heavy, were filed.

While these bullets were making, the thermometer exposed to the open air was at the freezing point, or some degrees below; but in the pit where the bullets were suffered to cool, the thermometer was nearly ten degrees above that point; that is to say, to the degree of temperature of the pits of the observatory, and it is this degree which I have here taken for that of the actual temperature of the earth. To know the exact moment of their cooling to this actual temperature, other bullets of the same matters, diameters, and not heated, were made use of for comparison, and which were felt at the same time as the others. By the immediate touch of the hand, or two hands, on the two bullets, we could judge of the moment when they were equally cold; and as the greater or less smoothness or roughness of bodies makes a great difference to the touch; (a smooth body, whether hot or cold, appearing much more so than a rough body, even of the same matter, although they are both equally so) I took care that the cold bullets

lets were rough, and like those which had been heated, whose surfaces were sprinkled over with little eminences produced by the fire.

EXPERIMENTS.

I. The bullet of half an inch was heated white in two minutes, cooled so as to be held in the hand in 12, and to the actual temperature in 39 minutes.

II. That of an inch, heated white in five minutes and a half, cooled so as to be held in the hand, in 35½ minutes, and to the actual temperature in one hour and 23 minutes.

III. That of an inch and an half, heated white in nine minutes, cooled so as to be held in the hand in 58 minutes, and to the actual temperature in two hours and 35 minutes.

IV. That of two inches heated white in 13 minutes, cooled so as to be held in the hand in one hour 20 minutes, and to the actual temperature in three hours 16 minutes.

V. That bullet of two inches and an half heated white in 16 minutes, cooled so as to be held in the hand in one hour 42 minutes, and to the actual temperature in four hours 30 minutes.

VI. That bullet of three inches heated white in 19½ minutes, cooled so as to be held in the hand in two hours seven minutes, and to the actual temperature in five hours eight minutes.

VII. That of three inches and a half heated white in 23½ minutes, cooled so as to be held in the hand in two hours 36 minutes, and to the actual temperature in five hours 56 minutes.

VIII. That of four inches heated white in 27 minutes and a half, cooled so as to be held in the hand in three hours two minutes, and to the actual temperature in six hours 55 minutes.

IX. That of four inches and a half heated white in 31 minutes, cooled so as to be held in the hand in three hours and 25 minutes, and to the actual temperature in seven hours 46 minutes.

X. That of five inches heated white in 34 minutes, cooled, so as to be held in the hand, in three hours 52 minutes, and to the actual temperature in eight hours 42 minutes.

The most constant difference that can be taken between each of the terms which express the time of cooling, from the instant the bullets were drawn from the fire, to that when we can touch them unhurt, is found to be about 24 minutes, for, by supposing each term to increase 24, we shall have 12, 36, 60, 84, 108, 132, 156, 180, 204, 228 minutes. And the continuation of the real time of these coolings are, 12, 35½, 58, 80, 102, 127, 156, 182, 205, 232 minutes, which approach the first as nearly as experiment can approach calculation.

So, likewise, the most constant difference to be found between each of the terms of cooling to actual temperature is found to be 54 minutes, for by supposing each term to increase 54, we shall have 39, 93, 147, 201, 255, 309, 363, 417, 471, 525 minutes, and the continuation of the real time of this cooling is found, by the preceding experiments, to be 39, 93, 145, 196, 248, 308, 356, 415, 466, 522 minutes, which approaches also nearest to the first.

I made the like experiments upon the same bullets twice or thrice, but found I could only rely on the first, because each time the bullets were heated they lost a considerable part of their weight, which was occasioned not only by the falling off of the parts of the surface reduced into scoria, but also by a kind of drying, or internal calcination, which diminishes the weight of the constituent parts, insomuch that it appears a strong fire renders the iron specifically lighter each time it is heated; and I have found, by subsequent experiments, that this diminution of weight varies much, according to the different quality of the iron. Experience has also confirmed me in the opinion, that the duration of heat, or the time

taken

taken up in cooling of iron, is not in a smaller, as stated in a passage of Newton, but in a larger ratio than that of the diameter.

Now if we would enquire how long it would require for a globe as large as the earth to cool, we should find, after the preceding experiments, that instead of 50,000 years, which Newton assigns for the earth to cool to the present temperature, it would take 42,964 years, 221 days, to cool only to the point where it would cease to burn, and 86,667 years and 132 days, to cool to the present temperature.

It might only be supposed, that the refrigeration of the earth should be considerably increased, because we imagine that refrigeration is performed by the contact of the air, and that there is a great difference between the time of refrigeration in the air and in vacuo; and supposing that the earth and air cool in the same time in vacuo, this surplus of time should be reckoned. But, in fact, this difference of time is very inconsiderable, for though the density of the medium, in which a body cools, makes something on the duration of the refrigeration, yet this effect is much less than might be imagined, since in mercury, which is eleven thousand times denser than air, it is only requisite to plunge bodies into it about nine times as often as is required to produce the same refrigeration in air. The principal cause of refrigeration is not, therefore, the contact of the ambient medium, but the expansive force which animates the parts of heat and fire, which drives them out of the bodies wherein they reside, and impels them directly from the centre to the circumference.

By comparing the time employed in the preceding experiments to heat the iron globes, with that requisite to cool them, we find that they may be heated till they become white in one sixth part and a half of the time they take to cool, so as to be held in the hand, and about one fifteenth and a half of that to cool to actual temperature, so that there is a great error in the estimate which Newton made on the heat communicated by the sun to the comet of 1680, for that comet having been exposed to the violent heat of the sun but a short time, could receive it only in proportion thereto, and not only in so great a degree as that author supposes. Indeed, in the passage alluded to, he considers the heat of red-hot iron much less than in fact it is, and he himself states it to be, in a Memoir, entitled, *The Scale of Heat*, published in the Philosophical Transactions of 1701, which was many years after the publication of his *principles.* We see in that excellent Memoir, which includes the germ of all the ideas on which thermometers have since been constructed; that Newton, after very exact experiments, makes the heat of boiling water to be three times greater than that of the sun in the height of summer; that of melted tin, six times greater; that of melted lead, eight times; that of melted regulus, twelve times; and that of a common culinary fire, sixteen or seventeen times; hence we may conclude, that the heat of iron, when heated so as to become white, is still greater, since it requires a fire continually animated by the bellows to heat it to that degree. Newton seems to be sensible of this, for he says, that the heat of iron in that state seems to be seven or eight times greater than that of boiling water. This diminishes half the heat of this comet, compared to that of hot iron.

But this diminution, which is only relative, is nothing in itself, nor nothing in comparison with that real and very great diminution which results from our first consideration. For the comet to have received this heat a thousand times greater than that of red-hot iron, it must have remained a very long time in the vicinity of the sun, whereas it only passed very rapidly at a small distance. It was on the 8th of December, 1680, at $\frac{6}{1000}$ distance from the earth to the centre of the sun; but 24 hours before, and as many after, it was at a distance six times greater, and where the heat was consequently 36 times less.

To know then the quantity of this heat communicated to the comet by the sun, we here find how we should make this estimation tolerably just, and, at the same time, make the comparison with hot iron by the means of my experiments.

We shall suppose, as a fact, that this comet took up 666 hours to descend from the point where it then was, and which point was at an equal distance as the earth is from the sun, consequently it received an equal heat to what the earth receives from that luminary, and which I here take for unity; we shall likewise suppose that the comet took 666 hours more to ascend from the lowest point of its perihelium to this same distance; and supposing also its motion uniform, we shall perceive, that the comet being at the lowest point of its perihelium, that is, to $\frac{6}{1000}$ of the distance from the earth to the sun, the heat it received in that motion was 27,766 times greater than that the earth receives. By giving to this motion a duration of 80 minutes, viz. 40 for its descent, and 40 for its ascent, we shall have, at 6 distance, 27,776 heat during 80 minutes at 7 distance 20,408 heat also during 80 minutes, and at 8 distance 15,625 heat during 80 minutes, and thus, successively, to the distance of 1000, where the heat is one. By summing up the quantity of heat at each distance we shall find 363,410 to be the total of the heat the comet has received from the sun, as much in descending as in ascending, which must be multiplied by the time, that is, by four thirds of an hour; we shall then have 484,547, which divided by 2,000 represents the solid heat the earth received in this time of 1332 hours, since the distance is always 1,300, and the heat always equals one. Thus we shall have 242,$\frac{547}{2000}$ for the heat the comet received more than the earth during the whole time of its perihelium instead of 28,000, as Newton supposed it, because he took only the extreme point, and paid no attention to the very small duration of time. And this heat must still be diminished 242,$\frac{547}{2000}$, because the comet ran, by its acceleration, as much more way in the same as it was nearer the sun. But by neglecting this diminution, and admitting that the comet received a heat nearly 242 times greater than that of our summer's sun, and, consequently 17$\frac{2}{7}$ times greater than that of hot iron, according to Newton's estimation, or only ten minutes greater according to this estimation; it must be supposed, that give a heat ten times greater than that of red hot iron, it required ten times more time; that is to say, 1332; consequently, we may compare the comet to a globe of iron heated by a forge fire for 13320 hours, to heat it to a whiteness.

Now we find by calculation from my experiments, that with a forge fire, we can heat to a whiteness a globe whose diameter is 228342½ inches in 799200 minutes, and, consequently, the whole mass of the comet to be heated to the point of iron to a whiteness, during the short time it was exposed to the heat of the sun, could only be 223342½ inches in diameter; and even then it must have been struck on all sides, and at the same time, by the light of the sun. Thus comets, when they approach the sun, do not receive an immense nor a very durable heat, as Newton says, and as we at the first view might be inclined to believe. Their stay is so short in the vicinity of the sun, that their masses have not time to be heated, and besides only part of their surface is exposed to it; this part is burnt by the extreme heat, which by calcining and volatilizing the matter of this surface, drives it outwardly in vapours and dust from the opposite side to the sun; and what is called the tail of the comet, is nothing else than the light of the sun rendered visible, as in a dark room, by those atoms which the heat lengthens as it is more violent.

But another consideration very different and infinitely more important, is, that to apply the result of our experiments and calculation to the comet and earth, we must suppose them composed of matters which would demand as much time as iron to cool: whereas, in reality, the principal matters of which the terrestrial globe is composed, such as clay, stones, &c. cannot possibly take so long.

To satisfy myself on this point, I caused globes of clay and marl to be made, and having heated them at the same forge until white, I found that the clay balls of two inches, cooled in 38 minutes so as to be held in the hand; those of two inches and an half, in 48 minutes; and those of three inches, in 60 minutes; which being compared with the time of the refrigeration of iron bullets of the same diameters, give 38 to 80 for two inches, 48 to 102 for two inches and a half, and 60 to 127 for three inches; so that only half the time is required for the refrigeration of clay, to what is necessary for iron.

I found also, that lumps of clay, or marl, of two inches, refrigerated so as to be held in the hand in 45 minutes; those of two inches and a half in 58; and those of three inches in 75, which being compared with the time of refrigeration of iron bullets of the same diameters, gives 46 to 80 for two inches, 58 to 102 for two inches and a half, and 75 to 127 for three inches, which nearly form the ratio of 9 to 5; so that for the refrigeration of clay, more than half the time is required than for iron.

It is necessary to observe, that globes of clay heated white, lost more of their weight than iron bullets, even to the ninth or tenth part of their weight: whereas marl heated in the same fire, lost scarcely any thing, although the whole surface was covered over with scales, and reduced into glass. As this appeared singular, I repeated the experiment several times, increasing the fire, and continuing it longer than for iron; and although it scarcely required a third of the time to redden marl, to what it did to redden iron, I kept them in the fire thrice as long as was requisite, to see if they would lose more, but I found very trifling diminutions;

minutions; for the globe of two inches heated for eight minutes, which weighed seven ounces, two drachms, and thirty grains, before it was put in the fire, lost only forty-one grains, which does not make a hundredth part of its weight; and that of three inches, which weighed twenty-four ounces, five drachms, and thirteen grains, having been heated by the fire for eighteen minutes, that is nearly as much as iron, lost only seventy-eight grains, which does not make the hundredth and eighty-first part of its weight. These losses are so trifling, that it may be looked upon, in general, as certain that pure clay loses nothing of its weight in the fire; for those trifling diminutions were certainly occasioned by the ferruginous parts which were found in the clay, and which were in part destroyed by the fire. It is also worthy of observation, that the duration of heat in different matters exposed to the same fire for an equal time, is always in the same proportion, whether the degree of heat be greater or smaller.

I have made similar experiments on globes of marble, stone, lead, and tin, by a heat only strong enough to melt tin, and I found, that iron refrigerated in eighteen minutes, so as to be able to hold it in the hand, marble refrigerated to the same degree in twelve minutes, stone in eleven, lead in nine, and tin in eight.

It is not, therefore, in proportion to their density, as is commonly supposed, that bodies receive and lose more or less heat, but in an inverse ratio of their solidity; that is, of their greater or lesser *non fluidity ;* so that, by the same heat, less time is requisite to heat or cool the most dense fluid.

To prevent the suspicion of vainly dwelling upon assertion, I think it necessary to remark upon what foundation I build this theory; I have found that bodies which should heat in ratio of their diameters, could be only those which were perfectly permeable to heat, and would heat or cool in the same time; hence, I concluded that fluids, whose parts are only held together by a slight connection, might approach nearer to this perfect permeability than solids, whose parts have more cohesion. In consequence of this, I made experiments, by which I found, that with the same heat all fluids, however dense they might be, heat and cool more readily than any solids, however light, so that mercury, for example, heats much more readily than wood, although it be fifteen or sixteen times more dense.

This made me perceive that the progress of heat in bodies cannot, in any case, be made relatively to their density; and I have found by experience, that this progress, as well in solids as fluids, is made rather by reason of their fluidity, or in an inverse ratio of their solidity. I mean by *solidity* the quality opposite to fluidity; and I say, that it is in an inverse ratio of this quality that the progress of heat is made in both bodies; and that they heat or cool so much the faster as they are the more fluid, and so much the slower as they are more solid, every other circumstance being equal.

To prove that solidity, taken in this sense, is perfectly independent of density, I have found, by experience, that the most or least dense matters, heat or cool more readily than other more or less dense matters, for example, gold or lead, which are much more dense than iron and copper, heat and cool much quicker; while tin and marble, which are not so dense, heat and cool much faster than iron and copper; and there are likewise many other matters which come under the same description; so that density is in no manner relative to the scale of the progress of heat in solid bodies.

It is likewise the same in fluids, for I have observed, that quicksilver, which is thirteen or fourteen times more dense than water, nevertheless heats and cools in less time than water;

and spirit of wine, which is less dense than water, heats and cools much quicker; so that generally the progress of heat in bodies, as well with regard to the ingress as egress, has no affinity with their density, and is principally made in the ratio of their fluidity, by extending the fluidity to a solid; from hence I concluded, that we should know the real degree of fluidity in bodies, by heating them to the same heat; for their fluidity would be in a like ratio as that of the time during which they would receive and lose this heat; and that it would be the same with solid bodies. They will be so much the more solid, that is to say, so much the more *non fluids*, as they require more time to receive and lose this heat, and that almost generally to what I presume; for I have already tried these experiments on a great number of different matters, and from them I have made a table, which I have endeavoured to render as complete and exact as possible.

I caused several globes to be made of an inch diameter with the greatest possible precision, from the following matters, which nearly represent the Mineral kingdom.

M. Tillet, of the Academy of Sciences, made the globe of refined gold at my particular request, and the whole of them weighed as follows:

	oz.	d.	gr.
Gold	6	2	17
Lead	3	6	28
Pure silver	3	3	22
Bismuth	3	0	3
Copper-red	2	7	56
Iron	2	5	10
Tin	2	3	48
Antimony melted, and which had small cavities on its surface	2	1	34
Fine	2	1	2
Emery	1	2	24½
White marble	1	0	25
Pure clay	0	7	24
Marble common of Montbard	0	7	20
White gypsum, improperly called Alabaster	0	6	36
Calcareous white stone of the quarry of Anieres, near Dijon	6	6	6
Rock chrystal: it was a little too small, and had many defects. I presume that without them it would have weighed	0	6	22
Common glass	0	6	21

	oz.	d.	gr.
Pure earth, very dry -	0	6	16
Oker - - -	0	5	9
Porcelain of the Court de Lauraguais - -	0	5	2½
White chalk - -	0	4	49
Cherrywood, which although lighter than most other woods, is that which takes in the least fire	0	1	59

I must here observe, that a positive conclusion must not be made of the exact specific weight of each matter from the preceding table, for notwithstanding the precaution that was taken to render the globes equal, yet, as I was obliged to employ different workmen, some were too large, and others too small. Those which were more than an inch diameter were diminished, but those of rock chrystal, glass and porcelain, which were rather too small, we suffered to remain, and only rejected those of agate, jasper, and porphyry, which were sensibly so. This precision in size was however not absolutely necessary, for it could very little alter the result of my experiments.

Previously to ordering these globes, I exposed to a like degree of fire, a square mass of iron, and another of lead of two inches diameter, and found, by reiterated essays, that lead heated and cooled in much less time than iron. I made the same experiment on red copper, and that required more time to heat and cool than lead, and less than iron. So that of these three matters, iron appeared the least accessible to heat, and, at the same time, that which retained it the longest. From which I learn that the law of the progress of heat in bodies was not proportionable to their density, since lead, which is more dense than iron or copper, nevertheless heats and cools in less time than either. As this object appeared important, I was induced to have these globes made, and to be more perfectly satisfied of the progress of heat in a great number of different matters, I always placed the globes at an inch distance from each other, before the same fire, or in the same oven, 2, 3, 4, or 5, together with a globe of tin in the midst of them. In most of my experiments I suffered them to be exposed to the same active fire till the globe of tin began to melt, and at that instant they were all removed, and placed on a table in small cases. I suffered them to cool without moving, often trying whether I could touch them, and the moment they left off burning, and I could hold them in my hands half a second, I marked the time which had passed since I drew them from the fire. I afterwards suffered them to cool to the actual temperature, of which I endeavoured to judge by means of touching other small globes of the same matters that had not been heated. Of all the matters which I put to the trial, there was only sulphur which melted in a less degree of heat than tin, and notwithstanding its disagreeeble smell I should have taken it for a term of comparison, but being a brittle matter which diminishes by friction, I preferred tin, although it required nearly double the heat to melt.

Having heated together bullets of iron, copper, lead, tin, gres, and Montbard marble, they cooled in the following order:

	So as to be held in the hand for half a second. Min.		*To actual temperature.* Min.
Tin in - - -	6½	In - - -	16
Lead in - -	8	In - - -	17
Gres in - -	9	In - - -	19
Common marble in	10	In - - -	21
Copper in -	11½	In - - -	30
Iron in - -	13	In - - -	38

By a second experiment with a fiercer fire, sufficient to melt the tin bullet, the five others cooled.

	So as to be held in the hand.		*To actual temperature.*
	Min.		Min.
Lead in	10½	In	42
Gres in	12½	In	46
Common marble	13½	In	50
Copper	19½	In	51
Iron	23½	In	54

By a third experiment, with a less degree of fire than the preceding, the same bullets with a fresh tin bullet, cooled in the following manner.

	So as to be held in the hand.		*To actual temperature.*
	Min.		Min.
Tin in	7½	In	25
Lead in	9½	In	25
Gres in	10½	In	37
Common marble	12	In	39
Copper	14	In	44
Iron	17	In	50

From these experiments, which I made with as much precision as possible, we may conclude, first, that the time of refrigeration of iron, so as to be held in the hand, is to that of copper : : 53½ : 45, and so to the point of temperature : : 142 : 125.

2dly, That the time of refrigeration of iron, so as to be held in the hand, is to that of the first refrigeration of common marble : : 53½ : 35½ and their entire refrigeration : : 142 : 110.

3dly, that the time of refrigeration of iron, to that of gres, so as to be held in the hand, is : : 53½ : 32 and : : 142 : 102½, for their entire refrigeration.

4thly, That the time of refrigeration of iron to that of lead, so as to be held in the hand, is : : 53½ : 27 and 142 : 94½ for their entire refrigeration.

In an oven hot enough to melt tin, although all the coals and cinders were drawn out, I placed, on a piece of iron wire, five bullets, distant from one another about nine lines, after which the oven was shut, and having drawn them out, in about 18 minutes they cooled,

So as to be held in the hand.		*To actual temperature.*	
	Min.		Min
Melted tin in	8	In	24
Silver in	14	In	40
Gold in	15	In	46
Copper in	16½	In	50
Iron in	18	In	56

In the same oven, but with a slower heat, the same bullets with an other bullet of tin, cooled,

So as to be held in the hand.		*To actual temperature.*	
	Min.		Min.
Tin in	7	In	20
Silver in	11	In	56

Gold

Gold in	12½	In	40
Copper in	14	In	43
Iron in	16½	In	47

In the same oven, but with a still less degree of heat, the same bullets cooled,

	So as to be held in the hand. Min.		*To actual temperature.* Min.
Tin in	6	In	17
Silver in	9	In	26
Gold in	9½	In	23
Copper in	10	In	31
Iron in	11	In	35

Having placed in the same oven five other bullets, placed the same and separated from each other, their refrigeration was in the following proportions.

	So as to be held in the hand. Min.		*To actual temperature.* Min
Antimony in	6½	In	25.
Bismuth in	7	In	26
Lead in	8	In	27
Zinc in	10½	In	30
Emery in	11½	In	38

In the same oven, and in the same manner, another bullet of Bismuth was placed, with six other bullets, which cooled,

	So as to be held in the hand. Min.		*To actual temperature.* Min
Antimony in	6	In	23
Bismuth in	6	In	25

Lead

Lead in - -	7½	In - - - -	28
Silver in - -	9½	In - - - -	30
Zinc in - -	10½	In - - - -	32
Gold in - -	11½	In - - - -	34
Emery in - -	13½	In - - - -	39

There was put in the same oven a bullet of glass, another of tin, one of copper, and one of iron, and they cooled,

So as to be held in the hand.		*To actual temperature.*	
	Min.		Min.
Tin in - -	8	In - - - -	27
Glass in - -	8½	In - - - -	22
Copper in - -	14	In - - - -	42
Iron in - -	16	In - - - -	50

Bullets of gold, glass, porcelain, gypsum, and gres, were heated together, and cooled,

So as to be held in the hand.		*To actual temperature.*	
	Min.		Min.
Gypsum in - -	8	In - - - -	24
Porcelain in - -	8½	In - - - -	25
Glass in - -	2	In - - - -	26
Gres in - -	10	In - - - -	32
Gold in - -	14½	In - - - -	45

Bullets of silver, common marble, hard stone, white marble, and soft calcareous stone of Anieres, near Dijon, were heated like the former, and cooled,

So

So as to be held in the hand.	Min.	*To actual temperature.*	Min.
Soft calcareous stone in	8	In - -	25
Hard stone in - - -	10	In - -	34
Common marble in -	11	In - -	35
White marble in - -	12	In - -	36
Silver in - - - - -	13½	In - -	40

The whole of these experiments were made with the utmost care and attention, not only by myself but in the presence of several persons, who also endeavoured to judge of the first degree of temperature by holding the bullets for half a second in their hands, and the relations of which are more exact than those of the actual temperature, because that being variable the result must sometimes vary also.

With a view to avoid that proxility which would necessarily attend the continual repetition in a comparative statement of the refrigeration of these different bodies, we have connected them in a general table, and taking 10,000 for the standard of comparison, their diffences may be seen at one view.

A TABLE

OF THE

Relations of different Mineral Substances.

IRON, with

		First Refrig.	Entire Refrig.
Emery	10000 to	9117	9020
Copper	—— to	8512	8702
Gold	—— to	8160	8148
Zinc	—— to	7653 6804	6020
Silver	—— to	7619	7423
Marble White	—— to	6774	6704
Marble common	—— to	6636	6746
Stone calcareous hard	—— to	6617	6274
Gres	—— to	5596	6926
Glass	—— to	5576	5805
Lead	—— to	5143	6482
Tin	—— to	4898	4921
Stone calcareous soft	—— to	4194	4659
Clay	—— to	4198	4490
Bismuth	—— to	3580	4081
Chalk	—— to	3086	3878
Gum	—— to	2325	2817
Wood	—— to	1890	1594
Pumice-stone	—— to	1627	1268

EMREY.

EMERY, with

		First Refrig.	Entire Refrig.
Copper	10000 to	8519	8148
Gold	—— to	8513	8560
Zinc	—— to	8390	7693
		7458	
Silver	—— to	7778	7895
Stone calcareous hard	—— to	7304	6963
Gres	—— to	6552	6517
Glass	—— to	5862	5506
Lead	—— to	5718	6643
Zinc	—— to	5658	6000
Clay	—— to	5185	5185
Bismuth	—— to	4949	6060
Antimony	—— to	4540	5827
Oker	—— to	4259	3827
Chalk	—— to	3684	4105
Gypsum	—— to	2368	2947
Wood	—— to	1552	3146

COPPER, with

Gold	10000 to	9136	9194
Zinc	—— to	8571	9250
		7619	
Silver	—— to	8395	7823
Marble common	—— to	7639	8019
Gres	—— to	7333	8160
Glass	—— to	6667	6567
Lead	—— to	6179	7367

Tin

		First Refrig.	Entire Refrig.
Tin	10000 to	5746	6916
Stone calcareous tender	—— to	5168	5633
Clay	—— to	5652	6363
Bismuth	—— to	5686	5959
Antimony	—— to	5130	5808
Oker	—— to	5003	4697
Chalk	—— to	4068	4368

GOLD, with

Zinc	10000 to	2474 8422	9304
Silver	—— to	8936	8686
Marble white	—— to	8101	7863
Marble common	—— to	7342	7434
Stone calcareous hard	—— to	7383	7516
Gres	—— to	7368	7627
Glass	—— to	7103	5232
Lead	—— to	6526	7500
Tin	—— to	6324	6051
Stone calcareous soft	—— to	6087	5811
Clay	—— to	5814	5077
Bismuth	—— to	5658	7043
Porcelain	—— to	5526	5593
Antimony	—— to	5395	6348
Oker	—— to	5349	4462
Chalk	—— to	4571	4452
Gypsum	—— to	2989	3293

ZINC, with

		First Refrig.	Entire Refrig.
Silver	10000 to	8904	8990
	10015		
Marble white	—— to	8305	8424
	7194		
Gres	—— to	6242	7333
	5838		
Lead	—— to	6051	7947
	4940		
Tin	—— to	6777	6240
	5666		
Stone calcareous soft	—— to	5536	7719
	4425		
Clay	—— to	5484	7458
	4373		
Bismuth	—— to	5343	7547
	4232		
Antimony	—— to	5246	6608
	4135		
Chalk	—— to	3729	5862
	2618		
Gypsum	—— to	3409	4261
	2298		

SILVER, with

Marble white	10000 to	8681	9200
Marble common	—— to	7912	9040
Stone calcareous hard	—— to	7436	8580
Gres	—— to	7361	7767

Glass

		First Refrig.	Entire Refrig.
Glass	10000 to	7230	7212
Lead	—— to	7154	9184
Tin	—— to	6176	6289
Stone calcareous soft	—— to	6178	6289
Clay	—— to	6034	6710
Bismuth	—— to	6308	8877
Porcelain	—— to	5556	5242
Antimony	—— to	5692	7653
Oker	—— to	5000	5668
Chalk	—— to	4310	5000
Gypsum	—— to	2879	3366
Wood	—— to	2253	1864
Pumice-stone	—— to	2059	1525

WHITE MARBLE, with

Marble common	10000 to	8992	9405
Stone hard	—— to	8594	9130
Gres	—— to	8286	8990
Lead	—— to	7604	5555
Tin	—— to	7143	6792
Stone calcareous soft	—— to	6792	7281
Clay	—— to	6400	6286
Antimony	—— to	6286	6792
Oker	—— to	5400	5571
Gypsum	—— to	4920	5116
Wood	—— to	2200	2857

COMMON

COMMON MARBLE, with

	First Refrig.	Entire Refrig.
Stone hard	10000 to 9483	9665
Gres	—— to 8767	9273
Lead	—— to 7671	8590
Tin	—— to 7424	6666
Stone soft	—— to 7327	7959
Clay	—— to 7272	7213
Antimony	—— to 6279	8333
Oker	—— to 6136	6393
Chalk	—— to 5581	6333
Wood	—— to 2500	3279

HARD CALCAREOUS STONE, with

Gres	10000 to 9268	9355
Glass	—— to 8710	8352
Lead	—— to 8571	7931
Tin	—— to 1095	7931
Stone soft	—— to 8000	8095
Clay	—— to 6190	6897
Oker	—— to 4762	5517
Wood	—— to 2195	4516

GRES, with

Glass	10000 to 9324	7959
Lead	—— to 8561	8950
Tin	—— to 7667	7633
Stone soft	—— to 7644	7193
Porcelain	—— to 7364	7059

Antimony

		First Refrig.	Entire Refrig.
Antimony	10000 to	73[illegible]	6[illegible]0
Gypsum	—— to	4568	5000
Wood	—— to	2368	4828

GLASS, with

Lead	10000 to	9318	8548
Tin	—— to	9107	8679
Clay	—— to	7938	7643
Porcelain	—— to	7692	8863
Oker	—— to	6280	6500
Chalk	—— to	6104	6195
Gypsum	—— to	4160	6011
Wood	—— to	2647	5514

LEAD, with

Tin	10000 to	8695	8333
Stone soft	—— to	8437	7192
Clay	—— to	7878	8536
Bismuth	—— to	8698	8750
Antimony	—— to	8241	8201
Oker	—— to	6060	7073
Chalk	—— to	5714	6111
Gypsum	—— to	4736	5714

TIN, with

Clay	10000 to	8823	9524
Bismuth	—— to	8889	9400
Antimony	—— to	8710	9156
Oker	—— to	5882	7619

Chalk

		First Refrig.	Entire Refrig.
Chalk	10000 to	6394	6842
Gypsum	—— to	4090	4912
STONE CALCAREOUS SOFT, with			
Antimony	10000 to	7742	9542
Chalk	—— to	7288	7312
Gypsum	—— to	4182	5211
CLAY, with			
Bismuth	10000 to	8870	9416
Oker	—— to	8400	8571
Chalk	—— to	7701	8000
Gypsum	—— to	5185	8055
Wood	—— to	3437	4545
BISMUTH, with			
Antimony	10004 to	9349	9572
Oker	—— to	8846	7380
Chalk	—— to	8020	9500
PORCELAIN, with			
Gypsum	10000 to	5301	6500
ANTIMONY, with			
Chalk	10000 to	8431	7391
Gypsum	—— to	3833	5476
OKER, with			
Chalk	10000 to	8954	8889
Gypsum	—— to	6364	9062
Wood	—— to	4074	5128

CHALK,

	First Refrig.	Entire Refrig.
CHALK, with		
Gypsum - -	10000 to 6667	7920
GYPSUM, with		
Wood - -	10000 to 8000	5260
Pumice-stone -	—— to 70[illegible]9	4560
WOOD, with		
Pumice-stone -	10000 to 8750	8182

Notwithstanding the assiduity I used in my experiments, and the care I took to render the relations exact, I own there are still some imperfections in the foregoing table; but the defects are trivial, and do not much influence the general results; for example, it will easily be perceived, that the relation of zinc to lead being 10,0000 to 6,051, that of zinc to tin should be less than 6,000, whereas it is found 6,777 in the table. It is the same with respect of silver to bismuth, which ought to be less than 6,308, and also with regard of lead to clay, which ought to be more than 8,000, but in the table is only 7,878. This difference proceeded from the leaden and bismuth bullets not being always the same; they melted, as well as those of tin and antimony, and, therefore, could not fail to produce variations, the greatest of which are the three I have just remarked. It was not possible for me to do better; the different bullets of lead, tin, bismuth, and antimony, which I successively made use of, were made in the same manner, but the matter of each might be somewhat different, according to the quantity of the alloy in the lead and tin, for I had pure tin only for the two first bullets; besides, there remains very often a small cavity in the melted bullet, and these little causes are sufficient to produce the little differences which may be remarked in the table.

On the whole, to draw from these experiments all the profit that can be expected, the matters which compose their object must be divided into four classes, viz. 1. Metals. 2 Semi-metals and Metallic Minerals. 3. Vitreous and Vitrescible Substances. And 4. Calcareous andCalcinable substances. Afterwards the matters of each class must be compared between themselves to discover the cause, or causes, or the order which follows the progress of heat in each, and then with each other, in order to deduce some general results.

First. The order of the six metals, according to their *density*, is tin, iron, copper, silver, lead, and gold; whereas the order in which they receive and lose their heat is tin, lead, silver, gold, copper, and iron; so that in tin alone it retains its place.

The progress and duration of heat in metals does not then follow the order of their density, except in tin, which being the least dense, is also that which soonest loses its heat; but the order of the five other metals demonstrates that it is in relation to their fusibility that they all receive and loose heat; for iron is more difficult to melt than copper, copper more than gold, gold more than silver, silver more than lead, lead more than tin; and therefore we may conclude that it is only by chance if the density and fusibility of tin be found so united as to place it in the last rank. Nevertheless, it would be advancing too much to pretend that we must attribute all to fusibility, and nothing to density. Nature never deprives herself of one of her properties in favour of another in an absolute manner; that is to say, in a mode that the first has not any influence on the second. Thus, density may be of some weight in the progress of heat; but we may safely affirm, that in the six metals it has very little comparatively with fusibility.

This fact was neither known to chemists nor naturalists; they did not even imagine that gold which is more than twice as dense as iron, nevertheless loses its heat near a third sooner. It is the same with lead, silver, and copper, which are all more dense than iron, and which, like gold, heat and cool more readily; for though the object of this second memoir was only refrigeration, yet the experiments of the one that preceded it demonstrate, that there is ingress and egress of heat in bodies, and that those which receive it most quickly also lose it the soonest.

If we reflect on the real principles of density, and the cause of fusibility, we shall perceive, that density depends absolutely on the quantity of matter which Nature places in a given space; that the more she can make it enter therein, the more density there will be, and that gold, in this respect, is of all substances, that which contains the most matter relatively to its volume. It is for this reason that it has been hitherto thought, that more time is required to heat or cool gold than other metals; and it is natural enough to suppose, that containing double or treble the matter in the same volume, double or treble time would be required to penetrate it with heat; nay this would be true, if in every substance the constituent parts were of the same figure and ranged the same. But in the most dense the molecules of matter are, probably, of a figure sufficiently regular not to leave very void places between them; in others which are not so dense, and their figures more irregular, more vacuities are left, and in the lighest, the molecules being few, and most likely of a very irregular figure, a thousand times more void is found than plenitude; for it may be demonstrated by other experiments, that the volume of even the most dense substance contains more void space thanfull matter.

Now, the principal cause of fusibility is the facility which the particles of heat find in separating these molecules of full matter from each other; let the sum of the vacuities be greater or less, which causes density or lightness, it is indifferent to the separation of the molecules which constitute the plenitude; and the greater or less fusibility depends entirely on the power of coherence which retains the massive parts united, and opposes itself more or less to their separation. The dilatation of the total volume is the first degree of the action of heat; and in different metals it is made in the same order as the fusion of the mass, which is performed by a greater degree of heat or fire. Tin, which melts the most readily, is also that which dilates the quickest; and iron, which is the most difficult of all to melt, is likewise that whose dilatation is the slowest.

After these general positions, which appear clear, precise, and founded on experiments that nothing can contradict, it might be imagined that ductility would follow the order of fusibility, because the greater or less ductility seems to depend on the greater or less adhesion of the parts in each metal; nevertheless, ductility seems to have as much connection with the order of density, as with that of their fusibility. I would even affirm that it is in a ratio composed of the two others, but that would be only by estimation, and a presumption which is, perhaps not founded; for it is not so easy to exactly determine the different degrees of fusibility, as those of density; and as ductility participates of both, and varies according to circumstances, we have not as yet acquired the necessary knowledge to pronounce affirmatively o this subject, though it is most certainly of sufficient importance to merit particular researches. The same metal when cold gives very different results to what it does when hot, although treated in the same manner. Malleability is the first mark of ductility; but that gives only an imperfect idea of the point to which ductility may extend; nor can simple lead, the most malleable metal, be drawn into such fine threads as gold, or even as iron, which is the least malleable. Besides we must assist the ductility of metals with the addition of fire, without

out which they become brittle: even iron, although the most robust, is brittle like the rest. Thus the ductility of one metal, and the extent of continuity which it can support, depend not only on its density and fusibility, but also on the manner and space in which it is treated, and of the addition of heat or fire which is properly given to it.

II. By comparing those substances which we term *semi-metals* and *metallic minerals*, which want ductility, we shall perceive, that the order of their density is emery, zinc, antimony and bismuth, and that in which they receive and lose heat, is antimony, bismuth, zinc, and emery; and which does not in any measure follow the order of their density, but rather that of their fusibility. Emery, which is a ferruginous mineral, although as dense again as bismuth, retains heat longer. Zinc, which is lighter than antimony or bismuth, retains heat longer than either. Antimony and bismuth, receive and keep it nearly alike. There are, therefore, semi-metals, and metallic minerals, which, like metals, receive and lose heat nearly in the same relation as their fusibility, and partake very little of their density.

But by joining the six metals, and the four semi-metals, or metallic minerals, which I have tried, we shall find the order of the densities of these ten mineral substances to be emery, zinc, antimony, tin, iron, copper, bismuth, silver, lead and gold. And that the order in which these substances heat and cool, is antimony, bismuth, tin, lead, silver, zinc, gold, copper, emery and iron, in which there are two things that do not appear to perfectly agree with the order of fusibility.

First, Antimony, which, according to Newton, should heat and cool slower than lead, since by his experiments it requires ten degrees of the same heat to fuse, of which eight are sufficient for lead; whereas by my experiments antimony is found to heat and cool quicker than lead. But it should be observed that Newton made use of the regulus of antimony, and that I employed only melted antimony in experiments. Now this regulus of antimony, or native antimony, is much more difficult to fuse than antimony which has already undergone a first fusion, therefore that does not make an exception to the rule. On the whole, I do not know what relation native antimony, or regulus of antimony, may have with the other matters I have heated and cooled; but I presume, from the experiments of Newton, that it heats and cools slower than lead.

Secondly, it is pretended, that zinc fuses more easily than silver, consequently it should be found before silver in the order indicated by experiments, if this order were in all cases relative to that of fusibility; and I own that this semi-metal seems, at the first glance, to make an exception to the law which is followed by all the others; but it must be observed, that the difference given by my experiments between zinc and silver is very trifling. The small globe of silver which I made use of was of the purest silver, without the least mixture of copper; but I had my doubts whether that of zinc were entirely free from copper, or some other metal less fusible; and therefore, after all my experiments, I returned the globe of zinc to M. Rouelle, a celebrated professor of chemistry, requesting him carefully to examine it, which having done, after several trials, he found a pretty considerable quantity of iron, or saffron of steel in it.

I have, therefore, had the satisfaction of seeing that not only my own supposition was well founded, but also that my experiments have been made with sufficient precision to evince a mixture. Thus zinc exactly follows the order of fusibility, like the other metals and semi-metals, in the progress of heat, and does not make any exception to the rule. It cannot,

not therefore, in general, be said that the progress of heat in metals, semi-metals, and metallic minerals, is in the same ratio, or even nearly to that of their fusibility.

III. The Vitrescible and Vitreous Matters, which I tried, being ranged according to their density, are, pumice-stone, porcelaine, oker, clay, glass, rock-chrystal, and gres, for I must observe, that although chrystal is not set down in the table of the weight of each matter but for six drachms 22 grains, it must be supposed one drachm heavier, because it was sensibly too small; and it was for this reason that I excluded it from the general table of relations; nevertheless, as the general result agrees with the rest, I can present the following as the order in which these different substances are cooled:

Pumice-stone, oker, porcelain, clay, glass, crystal and gres, is according to that of their density, for the oker is here before the porcelain only because, being a fusible matter, it diminished by the friction it underwent in the experiments, and, besides, their density differs so little that they may be looked upon as equal.

Thus the law of the progress of heat in vitrescible and vitreous matters is relative to the order of their density, and has little or no relation with their fusibility but by the heat required to fuse these substances being in an almost equal degree, and the particular degree of their different fusibility being so near each other that an order of distinct terms cannot be made; thus their almost equal fusibility making only one term, which is the extreme of this order, we must not be astonished that the progress of heat here follows the order of density, and that these different substances, which are all equally difficult to fuse, heat and cool more or less quick in proportion to the matter they contain.

It may be objected to me that glass fuses more easily than clay, porcelain, oker, and pumice-stone, which, nevertheless, heat and cool in less time than glass; but the objection will fail when we reflect, that to fuse glass it is requisite to have a very fierce fire, the heat of which is so remote from the degrees which glass receives in our experiments on refrigeration, that it cannot have any influence on them. Besides, by powdering clay, porcelain, and pumice-stone, and by giving them their analogous fusers, as we give to sand to convert it into glass, it is more than probable that we should fuse all the matters in the same degree of fire, and that, consequently we must look upon it as equal, or almost equal, with their resistance to fusion; and it is for this reason that the law of the progress of heat in these matters is found proportionable to the order of their density.

IV. Calcareous matters, ranged according to the order of their density, are, chalk, soft stone, hard stone, common marble, and white marble, which is the same as that of their density. The fusibility is not here of any weight, because it requires at first a very great degree of fire to calcine them; and although the calcination divides the parts, we must look upon the effect only as a first degree and not as a complete fusion. The whole power of the best burning mirrors is scarcely sufficient to perform it. I have melted and reduced into a kind of glass some of these calcareous matters; and I am convinced that these matters may, like all the rest, be reduced ulteriorly into glass, without employing for this purpose any fusing matter, and only by the force of a fire superior to that of our furnaces; consequently the common term of their fusibility is still more remote, and more extreme, than that of vitreous matters, and it is for this reason that they also follow more exactly the order of density in the progress of heat.

White gypsum, improperly called alabaster, is a matter which calcines like all other plasters by a more moderate heat than that which is necessary for the calcination of calcareous matters, and it follows the order of density in the progress of heat which it receives or loses, for although much more dense than chalk, and a little more so than white calcareous stone, it heats and cools more readily than either of those matters. This demonstrates that the more or less easy calcination and fusion produces the same effects relatively to the progress of heat. Gypsous matters do not require so much fire to calcine as calcareous, and it is for this reason that, although more dense, they heat and cool much quicker.

Thus it may be concluded, that, in general, the *progress of heat in all Mineral Substances is always nearly in a ratio of their greater or less facility to calcine, or melt*: but that when their calcination, or their fusion, are *equally difficult*, and *that they require a degree of extreme heat*, then the *progress of heat is made according to the order of their density.*

I have deposited in the Royal Cabinet the globes of gold, silver, and of all the other metallic and mineral substances which served for the preceding experiments, that if the truth of their results, and the general consequences which I have deduced, be doubted, there may be an opportunity of rendering them more authentic.

OBSERVATIONS ON THE NATURE OF PLATINA.

WE have already seen, that of all the Mineral substances which I subjected to trial it was not the most dense, but the least fusible, which required the longest time to receive and lose heat. Iron and emery, which are the most difficult matters to fuse, are, at the same time, those that heat and cool the slowest. There is nothing except platina that is accessible to heat, which retains it longer than iron. This mineral, (which has not long been publicly mentioned) appears, however, to be more difficult to fuse; the fire of the best furnaces is not fierce enough to produce that effect, nor even to agglutinate the small grains, which are all angular, hard, and similar in form to the thick scale of iron, but of a yellowish colour; and although we can fuse them without any addition, and reduce them into a mass by a mirror, platina seems to require more heat than the ore and scales of iron which we easily fuse in our forge furnaces. In other respects, the density of platina being much greater than that of iron, the two quantities of density and non-fusibility unite here to render this matter the least accessible to the progress of heat. I presume, therefore, that platina would have been at the head of my table if I had put it to the experiment; but I was not able to procure a globe of it of an inch diameter, it being only found in grains*; and that which is in the mass is not pure, it being necessary, in order to fuse it, to mix it with other matters, which alter its nature. The Comte de Billarderie d'Angivilliers, who often attended my experiments, led me to examine this rare metallic substance, not yet sufficiently known. Chemists who have employed their time in platina, have looked upon it as a new, perfect, proper, and particular metal, different from all the rest: they have asserted, that its specific weight was nearly equal to that of gold; but that it essentially differed in other respects from gold, having neither ductility nor fusibility. I own I am of a quite contrary opinion; because a matter which has neither ductility nor fusibility, cannot rank in the number of metals, whose essential and common properties are to be ductile and fusible. Neither, after a very careful examination, did platina appear to me a new metal different from every other, but rather an alloy of iron and gold formed by Nature, in which the quantity of gold predominated over the iron; and I founded this opinion on the following facts:

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* I have been assured, however, by a person of the first respectability, that platina is sometimes found in masses, and that he himself saw a piece that weighed twenty pounds, pure as it was extracted from the mine.

Of 8 ounces 85 grains of platina, furnished me by Comte d'Angivilliers, which I presented to a strong loadstone, there remained only 1 ounce, 1 dram, and 98 grains, all the rest was taken away by the loadstone; therefore, nearly six-sevenths of the whole was attracted by the loadstone, which is so considerable a quantity, that it is impossible to suppose that iron is not contained in the intimate substance of platina, but that it is even there in a very great quantity. I am convinced it contains much more, for if I had not been weary of these experiments, which took me up several days, I should have attracted a great part of the remainder of the 8 ounces by my loadstone, for to the last it continued to draw some grains one by one, and sometimes two. There is, therefore, much iron in platina, and it is not simply mixed with it, as with a foreign matter, but intimately united and making part of its substance;

stance; or, if this is denied, it must be supposed, that there exists a second matter in Nature which like iron may be attracted by the loadstone.

All the platina I have had an opportunity of examining, has appeared to be mixed with two different matters, the one black, and very attractable by the loadstone; the other in larger grains, of a pale yellow, and much less magnetic than the first. Between these two matters, which are the two extremes, are found all the intermediate links, whether with respect to magnetism, colour, or size of the grains. The most magnetic, which are at the same time the blackest and smallest, reduce easily into powder by a very slight friction, and leave on white paper the same marks as lead. Seven leaves of paper which were successively made use of to expose the platina to the action of the loadstone, were blackened over the whole extent occupied by it; the last left less than the first, in proportion as the grains which remained were less black and magnetic; the largest grains, which are yellow, and least magnetic, instead of crumbling into powder like the small black grains, are very hard, and resist all trituration; nevertheless, they are susceptible of extension in an agate mortar, under the reiterated strokes of a pestle of the same matter,

ter, and I flattened and extended many grains to the double or treble extent of their surface: this part of platina, therefore, has a certain degree of malleability, and ductility, whereas the black part appears to be neither malleable nor ductile. The intermediate grains participate of the qualities of the two extremes: they are brittle and hard, they break or extend under the strokes of the pestle, and afford a little powder not so black as the first.

Having collected this black powder and the most magnetic grains that the loadstone at first attracted, I discovered that the whole was iron, but in a different state from common iron. The latter reduced into powder and filings contracts moisture, and rusts very readily; in proportion as the rust increases, it becomes less magnetic, and absolutely loses this magnetical quality when entirely and intimately rusted; whereas this iron powder, or ferruginous sand found in the platina, is inaccesssible to rust, how long soever it may be exposed to the air and humidity; it is also more infusible and much less dissoluble than common iron; but is, nevertheless, an iron which appears to differ only from common iron by a greater purity. This sand is, in fact, iron divested of all the combustible matter and all terrene parts which are found in common iron, and even in steel. It appears endowed and covered with a vitreous varnish which defends it from all injury. What is very remarkable, this pure iron sand does not exclusively belong to the platina ore; for I have found it, although always in small quantities, in many parts where the iron ore has been dug, and which consumed in my forges. As I submitted to several trials all the ores I had, before I used them in my experiments, I was surprised to find in some of them, which were in grains, particles of iron, somewhat rounded and shining, like the filings of iron, and perfectly resembling the ferruginous sand of the platina; they were all as magnetic, all as little fusible, and all as difficult of solution. Such was the result of the comparison I made on the sand of platina, and of the sand found in both my iron ores, at the depth of three feet, in earths where water easily penetrated. I was puzzled to conceive whence these particles of iron could proceed, how they had been defended against rust for the ages they were exposed to the humidity of the earth, and how this very magnetical iron had been produced in veins of mines, which had not the smallest degree of that quality. I called experience to my aid, and became at length satisfied upon these points. I was well convinced that none of our iron ores in grain were tractable by the loadstone, and well persuaded that all iron ores, which are magnetical, have acquired that property only by the action of fire: that the mines of the north, which are so magnetical as to be sought after by the compass, must owe their origin to fire, and are formed by the means, or the intermedium of water; from which I was induced to suppose that this ferruginous and magnetic sand, that I found in a small quantity in my iron mines, must owe its origin to fire, and having examined the place I was confirmed in this idea. This magnetical sand is found in a wood, where, from time immemorial, they have made, and still continue to make, coal furnaces. It is likewise more than probable that there were formerly considerable fires here. Coal and burnt wood produce iron dross, which includes the most fixed parts of iron that vegetables contain; it is this fixed iron which forms the sand here spoken of, when the dross is decomposed by the action of the air, sun, and rain, for then these pure iron particles, which are not subject to rust, nor to any other kind of alteration, suffer themselves to be carried away by the water, and penetrate with it some feet deep into the earth. What I here advance may be verified by grinding the dross well burnt, and there will be found a small quantity of this pure iron, which, having resisted the action of the fire, equally resists that of the solvents, and does not rust at all.

Being satisfied on this head, and having sufficiently compared the sand and dross taken from the iron ores with that of the platina, so as to have no doubt of their identity, it was not long before I was led to conclude, considering the specific gravity of platina, that if this pure iron sand, (proceeding from the decomposition of dross) instead of being in an iron mine, was found near to a gold one, it might, by uniting with that metal, form an alloy which would be absolutely of the same nature as platina. Gold and iron have a great affinity; and it is well-known that most iron mines contain a small quantity of gold; it is also known how to give to gold the tint, colour, and even the brittleness of iron, by fusing them together. This iron-coloured gold is used on different golden jewels to vary the colours; and this gold mixed with iron is more or less grey, and more or less tempered, according to the quantity of iron which enters the mixture. I have seen it of a tint absolutely like the colour of platina; and having enquired of a goldsmith the proportion of gold and iron therein, he informed me, that in a piece of 24 carats, there were no more than 18 gold, consequently a fourth part was iron, which is nearly the proportion found in the natural platina, if we judge of it by the specific weight; and this gold made with iron is harder and specifically less weighty than pure gold. All these agreements and common qualities with platina, have persuaded me, that this pretended metal is, in fact, only an alloy of gold and iron, and not a particular substance, a new and perfect metal different from every other, as chemists have supposed.

It is well known that alloy makes all metals brittle, and that when there is a penetration, that is, an augmentation in the specific gravity, the alloy is so much the more tempered as the penetration is the greater, and the mixture becomes the more intimate, as is perceived in the alloy called *bell-metal*, although it be composed of two very ductile metals. Now nothing is more tempered, nor heavier than platina, which alone ought to make us conclude that it is only an alloy made by Nature, a mixture of iron and gold, owing in part its specific gravity to this last, and, perhaps, also, in a great part, to the penetration of the two matters of which it is composed.

As this matter, heated alone and without any addition, is very difficult to reduce into a mass, as by the fire of a burning mirror we can obtain only very small masses, and as the hydrostatical experiments made on small volumes are so defective, that we cannot conclude any thing therefrom, it appears to me that the chemists have been deceived in their estimation of the specific gravity of this mineral. I put some powder of gold in a little quill, which I weighed very exactly; I put in the same quill an equal volume of platina, and it weighed nearly a tenth less; but this gold powder was much too fine in comparison of the platina. M. Tillet, who besides a profound knowledge of metals, possessed the talent of making experiments with the greatest precision, repeated, at my request, this experiment upon the specific weight of the platina, compared to pure gold; for this purpose, he, like me, made use of a quill, and cut gold of 24 carats, reduced as much as possible to the size of the grains of platina, and he found, by eight experiments, that the weight of platina differed from that of pure gold very near a fifteenth? but we both observed that the grains of gold had much shaper angles than the platina: all the angles of the latter were blunt, and even soft, whereas the grains of this gold had sharp and cutting angles, so that they could not adjust themselves, nor heap one on the other as easily as those of platina. The gold powder I had before made use of was such as is found in river sand, whose grains adjust themselves much better one against the other, and I found a about a tenth difference between the specific weight of those and platina; nevertheless, those are not pure gold, more than two or three carats being often wanting, which must diminish the specific weight in the same relation. Thus we have thought we might maintain, from the result of my experiments, that platina in grains, and such as Nature produces it, is, at least, an eleventh, or twelfth, lighter than gold. There is every reason to presume that the error on the density of platina, proceeded from its not having been weighed in its natural state, but only after it had been reduced into a mass; and as this fusion cannot be made but by the addition of other matters, and a very fierce fire, it is no longer pure platina, but a composition in which fusing matters are entered, and from which fire has taken the lightest parts.

Platina, therefore, instead of being of a density almost equal to that of pure gold, as has been asserted, is only a density between that of gold and iron, and only nearer this first metal than the last. For supposing that the cube foot of gold weighed 1326lb and that of iron 280, that of platina in grains will be found to weigh about 1194lb. which supposes more than $\frac{3}{4}$ of gold to $\frac{1}{4}$ of iron in this alloy, if there is no penetration; but as we extract $\frac{6}{7}$ by the loadstone, it might be thought, that there is more than $\frac{1}{4}$ iron therein: especially as by continuing this experiment, I am persuaded, we should be able, with a strong loadstone to bring away all the platina even to the last grain. Nevertheless, we must not conclude that iron is contained therein in so great a quantity; for when it is mixed by the fusion with gold, the mass which results from this alloy is attractable by the loadstone, although the iron is in no great quantity therein. M. Baume had a piece of this alloy weighing 66 grains, in which was only entered 6 grains, that is, $\frac{1}{11}$ of iron, and this button was easily taken up by the loadstone. Hence the platina might possibly contain only $\frac{1}{11}$ iron, or $\frac{16}{11}$ gold, and yet be attracted entirely by the loadstone; and this perfectly agrees with the specific weight which is $\frac{1}{12}$ less than gold.

But what makes me presume, that platina contains more than $\frac{1}{11}$ of iron, or $\frac{16}{11}$ of gold, is, that the alloy from this proportion is still of the gold colour, and much yellower than the highest coloured platina, and that $\frac{1}{4}$ iron, or $\frac{3}{4}$ gold is requisite for the alloy to be precisely of the natural colour of platina. I am, therefore, greatly inclined to think that there might possibly be this quantity of $\frac{1}{4}$ iron in platina. We were assured by many experiments, that the sand of this pure iron which contained platina, is heavier than the filings of common iron. Thus, this cause, added to the effect of penetration, is sufficient for the reason of this great quantity of iron contained under the small volume indicated by the specific weight of platina.

On the whole, it is very possible that I may be deceived in some of the consequences which I have drawn from my observations on this metallic substance: for I have not been able to make so profound an examination as I could wish; and what I say is only what I have observed, which may perhaps serve as a stimulus to other and better researches.

Chance led me to tell my ideas to Conte de Milly, who declared himself nearly of my opinion. I gave him the preceding remarks to inspect, and two days after he favoured me with the following observations, and which he has permitted me to publish.

"I weighed exactly thirty-six grains of platina; I laid them on a sheet of white paper that I might observe them the better with a magnifying glass: I perceived three different substances; the first had the metallic lustre, and was the most abundant; the second, drawing a little on the black, very nearly resembled a ferruginous metallic matter, which could undergo a considerable degree of fire, such as the scoria of iron, vulgarly called *machefer*: the third less abundant than the two first, i. e. sand, where the yellow, or topaz colour, is the most predominant. Each grain of sand, considered separate, offered to the sight regular chrystals of different colours. I remarked some in an hexagon form, terminating in pyramids like rock chrystal; and this sand seems to be no other than a *detritus* of chrystal, or quartz of different colours.

"I resolved on separating, as exactly as possible, these different substances, by means of the loadstone, and to put aside the parts most attractable by the loadstone, from those which were less, and both from those which were not so at all; then to examine each substance particularly, and to submit them to different chemical and mechanical heats.

"I separated these parts of the platina which were briskly attracted at the distance of two or three lines; that is to say, without the contact of the loadstone; and for this experiment I made use of a good fictious magnet; I afterwards touched the metal with this magnet, and carried off all that would yield to the magnetical force. Being scarcely any longer attractable, I weighed what remained, and which I shall call No. 4; it was twenty-four grains; No. 1, which was the most sensible to the magnet, weighed four grains; No. 2 weighed the same; and No. 3, five grains

"No. 1, examined by the magnifying glass, presented only a mixture of metallic parts, a white sand bordering on the greyish, flat and round, or black vitriform sand, resembling pounded scoria, in which very rusty parts are perceptible: in short, such as the scoria of iron presents after having been exposed to moisture.

"No. 2 presented nearly the same, excepting that the metallic parts predominated, and that there were very few rusty particles.

"No. 3 was the same, but the metallic parts were more voluminous; they resembled melted metal which had been thrown into water to be granulated; they were flat, and of all sorts of figures, rounded on the corners.

"No. 4, which had not been carried off by the magnet (but some parts of which still afforded marks of sensibility to magnetism, when the magnet was moved under the paper where they were in), was a mixture of sand, metallic parts, and real scoria, friable between the fingers, and which blackened in the same manner as common scoria. The sand seemed to be composed of small rock, topaz, and cornelian chrystals. I broke some on a steel, and the powder was like varnish, reduced into powder; I did the same to the scoria; it broke with the greatest facility, and presented a black powder which blackened the paper like the common.

"The metallic parts of this last (No. 4) appeared more ductile under the hammer than those of No. 1, which made me imagine they contained less iron than the first: from whence it follows, that platina may possibly be no more than a mixture of iron and gold made by Nature, or perhaps by the hands of men.

"I endeavoured to examine, by every possible means, the nature of platina: to assure myself of the presence of iron of platina by chemical means, I took No. 1, which was very attractable by the magnet, and No. 4, which was not; I sprinkled them with fuming spirit of nitre; I immediately observed it with the microscope, but perceived no effervescence: I added

I added distilled water thereon, and it still made no motion, but the metallic parts acquired new brilliancy, like silver: I let this mixture rest for five or six minutes, and having still added water, I threw some drops of alkaline liquor saturated with the colouring matter of Prussian blue, and very fine Prussian blue was afforded me on the first.

"No. 4, treated in the same manner, gave the same result. There are two things very singular to remark in these experiments; first, that it passes current among chemists who have treated on the platina, that aquafortis, or spirit of nitre, has no action on it. Yet, as I have just observed, it dissolves it sufficiently, though without effervescence, to afford Prussian blue, when we add the alkaline liquor phlogisticated and saturated with the colouring matter, which, as is known, participates iron into Prussian blue.

"Secondly, Platina, which is not sensible to the magnet, does not contain less iron, since spirits of nitre dissolves it enough, and without effervescence, to make Prussian blue. Whence it follows, that this substance, which modern chemists, perhaps too greedy of the marvellous, and too willing to give something novel, have considered as a ninth metal, may possibly be only a mixture of gold and iron.

"Without doubt there still require many experiments to determine how this mixture has taken place, if it be the work of Nature or the effects of some volcano, or simply the produce of the Spaniards' labours in the New World to acquire gold in the mines of Peru.

"If we rub platina on white linen it blackens it like common scoria, which made me suspect that it was the parts of iron reduced into scoria which are found in this platina, and give it this colour, and which seem, in this state, only to have undergone the action of a violent fire. Besides, having a second time examined platina with my lens, I perceived therein different globules of liquid mercury, which made me suppose that platina might be the produce of the hands of man, in the following manner:—Platina, as I have been told, is taken out of the oldest mines in Peru, which the Spaniards explored after the conquest of the New World. In those dark times only two methods were known of extracting gold from the sands which contained it; first, by an amalgama with mercury; secondly, by drying it. The golden sand was triturated with quicksilver, and when that was judged to be loaded with the greatest part of the gold, the sand was thrown away, which was named *crasse*, as useless and of no value.

"The other method was adopted with as little judgment; to extract it they began by mineralising auriferous metals by means of sulphur, which has no action on gold, the specific weight being greater than that of other metals: but to facilitate its precipitation iron was added, which loaded itself with the superabundant sulphur, and this method is still followed. The force of fire vitrifies one part of the iron, the other combines itself with a small portion of the gold, or even silver, which mixes with the scoria, from whence it cannot be drawn but by strong fusions, and being well instructed in the suitable intermediums which are made use of. Chemistry, which is now arrived to great perfection, affords, in fact, means to extract the greatest part of this gold and silver: but at the time when the Spaniards explored the mines of Peru, they were, doubtless, ignorant of the art of mining with the greatest profit; besides, they had such great riches at their disposal that they, probably, neglected the means which would have cost them trouble, care, and time; there is much reason therefore to conclude that they contented themselves with a first fusion, and threw away the scoria as useless, as well as the sand which had escaped the quicksilver, and perhaps they made a mere heap of these two mixtures, which they regarded as of no value.

"These scoria contained gold and silver, iron under different states, and that in different proportions unknown to us, but which, perhaps, are those that gave origin to the platina. The globules of quicksilver which I observed, and those of gold which I distinctly saw, with the assistance of a good lens, in the platina I had in my hands, have given birth to the ideas which I have written on the origin of this mineral; but I only give them as hazardous conjectures. To acquire some certainty we must know precisely where the platina mines are situated, and examine if they have been anciently explored, whether it be extracted from a new soil, or if the mines be only rubbish, and to what depth they are found; and, lastly, if they have any appearance of being placed by the hands of man there or not, which alone can verify or destroy the conjectures I have advanced." *

These observations of Comte de Milly confirm mine in almost every point. Nature is the same, and presents herself always the same to those who know how to observe her: thus we must

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* Baron Siekengen, minister of the elector Palatine, told M. de Milly, that he had then in his possession two memoirs which had been given to him by M. Kellner, chemist and metallurgist in the service of the Prince of Birckenfeld, at Manheim, and which offered to the court of Spain to return nearly as much gold as they would send him platina.

must not be surprized that, without any communication, we observed the same things, and deduced the same consequence therefrom; that platina is not a new metal, different from every other, but a mixture of iron and gold. To reconcile his observations still more with mine, and to enlighten, at the same time, the doubts which remain on the origin and formation of platina, I have thought it necessary to add the following remarks:

1. The Comte de Milly distinguishes three kinds of matters in platina, namely, two, metallic, and the third, non-metallic, of a chrystalline form and substance. He observed, as well as I, that one of the metallic matters is very attractable by the magnet, and the other but little, or not at all. I mentioned these two matters as well as he, but I did not speak of the third, which is not metallic, because there was none, or very little, on the platina on which I made my observations. It is possible that the platina which the Comte made use of was not so pure as mine, which, I observed with the greatest care, and in which I saw only some small transparent globules, like white melted glass, which were united to the particles of platina, or ferruginous sand, and which were carried any where by the magnet.

These transparent globules were very few, and in eight ounces of platina which I narrowly inspected with a very strong lens, I never perceived regular crystals. It rather appeared to me that all the transparent particles were globulous, like melted glass, and all attached to metallic parts; nevertheless, as I did not in the least doubt the veracity of the Comte de Milly's observation, who observed crystalline particles of a regular form, and in a great number, in his platina, I thought I ought not to confine myself solely to the examination of that platina of which I have spoken; and finding some in the king's cabinet, M. Daubenton and I examined it together: this appeared to be much less pure than that we had before made our experiments on; and in it we remarked a great number of small prismatic and transparent crystals, some of a ruby colour, others of a topaz, and others perfectly white, which convinced us of the correctness of the Comte de Milly in his observations; but this only proves that there are some mines of platina much more pure than others, and that in those which are the most so, none of these foreign bodies are found. M. Daubenton also remarked some grains flat at bottom and rough at top, like melted metal cooled on a plain, and I very distinctly saw one of these hemispherical grains, which might indicate that platina is a matter that has been melted by the fire; but it is very singular, that in this matter, if melted by fire, small crystals, topaz, and rubies, are found; and I know not whether we ought not to suspect fraud in those who supplied this platina, who, to increase the quantity, mixed it with these crystalline sands, for I never met with these crystals but in one half pound of platina given me by the Comte de Angilliviers.

2. I, as well as Comte de Milly, found gold sand in platina; it is readily discovered by its colour, and because it is not magnetical; but I own that I never perceived the globules of mercury which he states to have done; yet I do not mean therefore to deny their existence, only that it appears to me that the sand of gold meeting with the globules of mercury, in the same matter, they might be soon amalgamated, and not retain the colour of gold, which I have remarked in all the gold sand that I could find in half a pound of platina; besides, the transparent globules, which I have just spoken of, resemble greatly the globules of live and shining mercury, insomuch that at the first glance it is easy to be deceived in them.

3. There were by no means so may tarnished and rusty parts in my first platina as in that of Comte de Milly's, nor was it properly a rust which covered the surface of those ferruginous particles, but a black substance produced by fire, and perfectly similar to that which covers the surface of burnt iron. But my second platina, that which I had from the royal cabinet, had a mixture of some ferruginous parts, which under the hammer were reduced into a yellow powder, and had all the characters of rust. This platina therefore of the royal cabinet, and that of Comte de Milly, resembling in every respect, it is probable that they proceeded from the same part, and by the same road. I even suspect that both had been sophisticated and mixed nearly one half with foreign crystalline and ferruginous rusty matters, which are not to be met with in the natural platina.

4. The production of Prussian blue by platina appears evidently to prove the presence of iron in those parts even of this mineral which are the least attractable to the magnet, and at the same time confirms what I have advanced on the intimate mixture of iron in its substance. The flowing of platina by spirits of nitre, also proves that although it has no sensible effervescence, this acid attracts the platina in an evident

manner; and the authors who have asserted the contrary, have followed their common track, which consists in looking on all actions as null which do not produce an effervescence. These second experiments of the Comte de Milly would appear to me very important, if they succeeded always alike.

5. We must however admit that many essential points of information are wanting to pronounce affirmatively on the origin of platina. We know nothing of the natural history of his mineral, and we cannot too greatly exhort those who are able to examine it on the spot, to make known their observations; and until that is done we must confine ourselves to conjectures, some of which appear only more probable than others. For example, I do not imagine platina to be the work of man. The Mexicans and Peruvians knew how to cast and work gold before the arrival of the Spaniards, and they were not acquainted with iron, which nevertheless they must have employed in a great quantity. The Spaniards themselves did not establish furnaces in this country when they first inhabited it to fuse iron. There is, therefore, every reason to conclude, that they did not make use of the filings of iron for the separation of gold, at least in the be-

ginning of their labours, which does not go above two centuries and a half back; a time much too short for so plentiful a production as platina, which is found in large quantities in many places.

Besides, when gold is mixed with iron, by fusing them together, we may always, by a chemical process, separate them, and extract the gold: whereas, hitherto, chemists have not been able to make this separation in platina, nor determine the quantity of gold contained in this mineral. This seems to prove, that gold is united with it in a more intimate manner than the common alloy, and that iron is also in it, in a different state from that of common iron. Platina, therefore, appears to me to be the production of nature, and I am greatly inclined to think, that it owes its first origin to the fire of volcanos. Burnt iron, intimately united with gold by sublimation, or fusion, may have produced this mineral, which having been at first formed by the action of the fiercest fire, will afterwards have felt the impression of water, and reiterated frictions, which have given it the form of blunt angles. But water alone might have produced platina; for supposing gold and iron divided as much as possible by the humid mode, their molecules, by uniting, will have formed the grains which compose it, and which from the heaviest to the lightest contain gold and iron; the proposition of the chemist who offers to render *nearly* as much gold as they shall furnish him with platina, seems to indicate, that there is, in fact, only $\frac{1}{11}$ of iron to $\frac{10}{11}$ of gold in this mineral, or possibly less. But the *nearly* of this chemist is perhaps a fifth, or fourth, and indeed, if he could realize his promise to a fourth, it would be doing a great deal, and no vain boast.

Being at Dijon the summer of 1773, the Academy of Sciences and Belles Letters, of which I have the honour to be a member, expressed a desire of hearing my observations on platina; and having complied, M. de Morveau resolved to make some experiments on this mineral; for which purpose I gave him a portion of that which I had attracted by the loadstone, and also some which I had found insensible to magnetism, requesting him to expose it to the strongest fire he could possibly make. Some time after, he sent me the following experiments, which he was pleased to subjoin to mine.

"Monsieur the Comte de Buffon, in a journey to Dijon, in the summer of 1773, having caused me to remark in half a drachm of platina, na, which M. de Baume had sent him in 1768, grains in form of buttons, others flatter, and some black and scaly; and having separated by the loadstone those which are attractable from those which appeared not so, I tried to form Prussian blue with both. I sprinkled the fuming nitrous acid on the non-attractable parts, which weighed $2\frac{1}{2}$ grains. Six hours after I put distilled water on the acid, and sprinkled alkaline liquor, saturated with a colouring matter; however there was not a single atom of blue, the platina had only a little more brightness. I alike sprinkled the fuming acid on the remaining platina, part of which was attractable, the same Prussian alkali precipitated a blue feculency, which covered the bottom of a pretty large bason. The platina, after this operation, shewed like the first. I washed and dried it, and found it had not lost $\frac{1}{4}$ of a grain, or $\frac{1}{138}$ part; having examined it in this state I perceived a grain of beautiful yellow, which was pure gold.

" M. de Fourcy had lately told the world, that the dissolution of gold was thrown down in a blue precipitate by the Prussian alkali, and had placed this circumstance in a table of affinity; I was tempted to repeat this experiment,

ment, and sprinkled, in consequence, the phlogisticated alkaline liquor in the dissolution of gold, but the colour of this dissolution did not change, which made me suspect that the dissolution of gold made use of by M. de Fourcy might possibly not have been so pure.

"At the same time the Comte de Buffon having given me a sufficient quantity of platina to make further assays, I undertook to separate it from all foreign bodies by a good front; and I have here subjoined the processes and their results.

EXPERIMENTS.

"I. Having put a drachm of platina, in a cupel, into a furnace, I kept up the fire two hours, when the covers sunk down, the supporters having run, nevertheless the platina was only agglutinated; it stuck to the cupel, and had left spots of a rusty colour. The platina was then tarnished, even a little black, and had only augmented a quarter of a grain in weight; a quantity very small in comparison with that which other chemists have observed. What surprised me still more was, that this drachm of platina, as well as that I used for other experiments, had been successively carried away by the loadstone

stone, and made a portion of $\frac{6}{7}$ of eight ounces, of which the Comte de Buffon has before spoken.

"II. Half a drachm of the same platina, exposed to the same fire in a cupel, was also agglutinated; I adhered to the cupel, on which it had left spots of a rusty colour; the augmentation of weight was found to be nearly in the same proportion, and the surface as black.

"III. I put this half drachm into a new cupel, but instead of a cover I placed over it a leaden crucible. This I kept in the most extreme heat for four hours; when it was cooled I found the crucible soldered to the support, and having broken it I perceived that nothing had penetrated into the internal part of the crucible, which appeared to be only more glossy than before. The cupel had preserved its form and position; it was a little cracked, but not enough to admit of any penetration; the platina was also not adherent to it, though agglutinated, but in a much more intimate manner than in the first experiments; the grains were less angular, the colour more clear, and the brilliancy more metallic. But what was the most remarkable during the operation, there issued from its surface, probably in the first moments of its refrigeration, three drops of water, one of which, that arose perfectly spherical, was carried up on a small pedicle of the vitreous and transparent matter. It was of an uniform colour, with a slight tint of red, which did not deprive it of any transparency; the smallest of the other two drops had likewise a pedicle, and the other none, but was only attached to the platina by its external surface.

"IV. I endeavoured to assay the platina, and for that intent put a drachm of the grains taken up by the loadstone into a cupel, with two drachms of lead. After having kept up a very strong fire for two hours, I found an adherent button, covered with a yellowish and spungeous crust of two drachms twelve grains weight, which announces that the platina had retained one drachm twelve grains of lead. I put this button into another cupel in the same furnace, observing to turn it, by which it only lost twelve grains in two hours; its colour and form were very little changed. The same piece of platina was put into Macquer's furnace, and a fire kept up for three hours, when I was obliged to take it out, because the bricks began to run. The platina was become more metallic, but it, nevertheless, adhered to the cupel, and this time it lost 34 grains. I threw it into the fuming nitrous acid to assay it, and there arising a little effervescence, I added distilled water thereon. The platina lost two grains, and I remarked some small holes, like those which its flying off might occasion.

"There then remained only 22 grains of lead in the platina. I began to form a hope of vitrifying this remaining portion of lead, for which purpose I put the same piece of platina into a new cupel, and by the care I took for the admission of air, and other precautions, the activity of the fire was so greatly augmented that it required a supply every eleven minutes; to this degree of heat we kept for four hours, and then permitted it to cool.

"I perceived the next morning that the leaden crucible had resisted, and that the supporters were only glazed by the cinders. I found a piece in the cupel, not adhering, of a uniform colour, approaching more the colour of tin than any other metal, but only a little ragged. It weighed exactly one drachm. All, therefore, announced that this platina had endured an absolute fusion, and that it was perfectly pure, for if we suppose it still contained lead, we must then admit that it had lost exactly as much of its substance as it had gained of foreign matter; and such a precision cannot be the effect of pure chance.

" I passed several days with M. Buffon, whose company has the same charms as his style, and whose conversation is as complete as his books: I took a pleasure in presenting him with the production of our essays; we examined them together, and observed, First, that the drachm of platina, agglutinated by these experiments, was not attractable by the loadstone; that, nevertheless, the magnetical bar had an action on the grains that were loosened from it.

" 2. The half drachm of the third experiment was not only attractable in the mass, but the grains of gold separated therefrom did not themselves give any signs of magnetism.

" 3. The platina of the fourth experiment was absolutely insensible to the loadstone.

" 4. The specific weight of this piece was determined by a good hydrostatical balance, and being, for the greater certainty, compared to coined and to other very pure gold, used by M. Buffon in his experiments, their density was found, with water, in which they were plunged,

Pure gold	-	$19 \frac{87}{1}$
Coin gold	-	$17 \frac{1}{2}$
Platina	-	$14 \frac{2}{5}$

" 5. This piece of platina was put upon

steel to try its ductability; it supported the hammer very well for a few strokes; its surface became flat and even, a little smooth in the parts which were struck, but it split soon after, and nearly a sixth part separated. The fracture presented many cavities, some of which had the whiteness and brilliancy of silver, and in others we remarked several points like chrystalization; the tops of these points examined with the lens, was a globule absolutely similar to that of the third experiment. All the other parts of this piece of platina were compact, the grain finer and closer than the best brass, which it resembled in colour. We offered several of these pieces to the loadstone, but not one was attracted thereby. We powdered them again in an agate mortar, and then remarked that the magnetical bar raised up some of the smallest every time they are placed under it.

"This new appearance of magnetism was so much the more surprising, as the grains were detached from the agglutinated mass of the second experiment, which seemed to have lost all sensibility at the approach and contact of the loadstone. In consequence we again took some of these grains, which were alike powdered, and soon perceived the smallest parts sensibly attach themselves to the magnetic bar. It is impossible

possible to attribute this effect to the smoothness of the bar, or to any other cause foreign to magnetism. A piece of smooth iron, applied in the same manner on the parts of this platina did not raise up a single grain.

"By these experiments, and the observations which have arisen therefrom, we may judge of the difficulty of determining the nature of platina. It is very certain that it contains some parts which are vitrifiable even without the addition of a fierce fire; it is also certain that all platina contains iron and attractable parts; but if the Prussian alkali never affords blue but with the grains which the loadstone attracts, we should conclude, that those which resist it are pure platina, which of itself has no magnetical virtue, and of which iron does not make an essential part. We must suppose that a sufficient fusion, or perfect cupellation, might decide the question; at least, these operations appear to have, in fact, deprived it of every magnetic virtue, by separating it from all foreign bodies; but the last observation proves, in an incontrovertible manner, that this magnetic property was, in reality, only weakened, and perhaps masked or buried, since it reappeared when it was ground."

From these experiments of M. de Morveau there results, 1. That we may expect to meet platina without addition, by applying the fire of it several times successively, because the best crucibles might not resist the action of so fierce a fire during the whole time that the complete operation would require.

2. That by melting it with lead, and assaying them several times, we should in the end vitrify all the lead and the platina; and that this experiment would be able to purge it from a part of the foreign matters it contains.

3. That by melting without any addition, it seems to purge itself partly into the vitrescible matters it includes, since it emits to its surface small drops of glass, which form pretty considerable masses, and which we can easily separate after refrigeration.

4. That by making experiments on Prussian blue with the grains of platina, which appeared to be most insensible to the loadstone, we were not always certain of obtaining it; a circumstance which never fails with grains that have more or less sensibility to magnetism.

5. It appears that neither fusion nor cupellation can destroy all the iron with which platina is intimately penetrated; the pieces melted or assayed, appeared, in reality, equally insensible to the action of the loadstone; but having pounded them in a mortar, we found magnetical

parts; so much the more abundant as the platina was reduced to a fine powder. The first piece, whose grains were only agglutinated, being ground, rendered many more magnetical parts than the second and third, the grains of which had undergone a stronger fusion; but, nevertheless, being both ground, they furnished magnetical parts; insomuch that it cannot be doubted that there is iron in platina, after it has undergone the fiercest efforts of fire, and the devouring actions of the heat in the cupel. This demonstrates, that this mineral is really an intimate mixture of gold and iron, which hitherto has not been able to separate.

6. I made another observation with M. Morveau on melted, and afterwards on ground platina; namely, that it takes in grinding precisely the same form as it had before it had been melted; all the grains of this melted and ground platina are similar to those of the natural, as well in form as variety of size; and they appear to differ only because the smallest alone suffer themselves to be raised by the loadstone, and in so much the less quantity as the platina has endured the fire. This seems also to prove, that, although the fire has been strong enough not only to burn and vitrify, but even to drive off a part of the iron with other vitrescible

matter which it contains; the fusion, nevertheless, is not so complete as that of other perfect metals, since, in grinding, it retakes the same figure as it had before fusion.

EXPERIMENTS ON LIGHT, AND ON THE HEAT IT MAY PRODUCE.

INVENTION OF MIRRORS TO BURN AT GREAT DISTANCES.

THE story of the burning glasses of Archimedes is famous; he is said to have invented them for the defence of his country; and he threw, say the ancients, the fire of the sun with such force on the enemy's fleet, as to reduce it into ashes as it approached the ramparts of Syracuse. But this story, which, for fifteen or sixteen centuries, was never doubted, has been contradicted, and treated as fabulous in these latter ages. Descartes, with the authority of a master, has attacked this talent attributed to Archimedes; he has denied the possibility of the invention, and his opinion has prevailed over the testimonies and credit of the ancients. Modern naturalists, either through a respect for their philosopher, or through complaisance for their contemporaries, have adopted the same opinion. Nothing is allowed to the ancients but what cannot be avoided. Determined, perhaps, by these motives, of which self-love too often is the abettor, have we not naturally too much inclination to refuse what is due to our predecessors? and if, in our time, more is refused than was in any other, is it not that, by being more enlightened, we think we have more right to fame, and more pretensions to superiority?

Be that as it may, this invention was the cause of many other discoveries of antiquity which are at present unknown, because the facility of denying them has been preferred to the trouble of finding them out; and the burning glasses of Archimedes have been so decried, that it does not appear possible to re-establish their reputation; for, to call the the judgment of Descartes in question, something more is required than assertions, and there only remained one sure decisive mode, but at the same time difficult and bold, which was to undertake to discover glasses that might produce the like effects.

Though I had conceived the idea, I was for a long time deterred from making the experiment, from the dread of the difficulty which might attend it; at length, however, I determined to search after the mode of making mirrors to burn at a great distance, as from 100 to 300 feet. I knew, in general, that the power of reflecting mirrors, never extended farther than 15 or 20 feet, and with refringent, the distance was still shorter: and I perceived it was impossible in practice to form a metal, or glass mirror, with such exactness as to burn at these great distances. To have sufficient power for that, the sphere, for example, must be 800 feet diameter; therefore, we could hope for nothing of that kind in the common mode of working glasses; and I perceived also that if we could even find a new method to give to large pieces of glass, or metal, a curve sufficiently slight, there would still result but a very inconsiderable advantage.

But to proceed regularly, it was necessary first to see how much light the sun loses by reflection at different distances, and what are the matters which reflect it the strongest; I first found, that glasses when they are polished with care, reflect the light more powerfully than the best polished metals, and even better than the compounded metal with which telescope mirrors are made; and that although there are two reflectors in the glasses, they yet give a brighter and more clear light than metal. Secondly, by receiving the light of the sun in a dark place, and by comparing it with this light of the sun reflected by a glass, I found, that at small distances, as four or five feet, it only lost about half by reflection, which I judged by letting a second reflected light fall on the first; for the briskness of these two reflected lights appeared to be equal to that of direct light. Thirdly, having received at the distances of 100, 200, and 300 feet, this light reflected by great glasses, I perceived that it did not lose any of its strength by the thickness of the air it had to pass through.

I afterwards tried the same experiments on the light of candles; and to assure myself more exactly of the quantity of weakness that reflection causes to this light, I made the following experiments:

I seated myself opposite a glass mirror with a book in my hand, in a room where the darkness of the night would not permit me to distinguish a single object. In an adjoining room I had a lighted candle placed at about 40 feet distance; this I approached nearer and nearer, till I could read the book, when the distance was about 24 feet. Afterwards turning the book, I endeavoured to read by the reflected light, having by a parchment intercepted the part of the light which did not fall on the mirror, in order to have only the reflected light on my book. To do so I was obliged to approach the candle nearer, which I did by degrees, till I could read the same characters clearly by the same light, and then the distance from the candle, comprehending that of the book to the mirror, which was only half a foot, I found to be in all 15 feet. I repeated this several times, and had always nearly the same results; from whence I concluded, that the strength, or quantity, of direct light is to that of reflected light, as 576 to 225; therefore, the light of five candles reflected by a flat glass, is nearly equal to that of the direct light of two.

The light of a candle, therefore, loses more by reflection than by the light of the sun; and this difference proceeds from the rays of the former falling more obliquely on the mirror than the rays of the sun, which come almost parallel. This experiment confirmed what I had at first found, and I hold it certain, that the light of the sun loses only half by its reflection on a glass mirror.

This first information being acquired, I afterwards sought what became of the images of the sun when received at great distances. To be perfectly understood we must not, as is generally done, consider the rays of the sun as parallel; and it must also be remembered, that the body of the sun occupies an extent of about 32 minutes; that consequently the rays which issue from the upper edge of the disk, falling on a point of a reflecting surface, the rays which issue from the lower edge falling also on the same point of this surface, they form between them an angle of 32 minutes in the incidence, and afterwards in the reflection, and that, consequently, the image must increase in size in proportion as it is farther distant. Attention must likewise be paid to the figure of those images; for example, a plain square glass of half a foot, exposed to the rays of the sun, will form a square image of six inches, when this image is received at the distance of a few feet; by removing farther and farther off, the image is seen to increase, afterwards to become deformed, then round, in which state it remains still increasing in size, in proportion as we are more distant from the mirror. This image is composed of as many of the sun's disks as there are physical points in the reflecting surface; the middle point forms an image of the disk, the adjoining points form the like, and of the same size, which exceed a little the middle disk: it is the same with the other points, and the image is composed of an infinity of disks, which surmounting regularly, and anticipating circularly one over the other, form the reflected image, of which the middle point of the glass is the centre.

If the image composed of all these disks is received at a small distance, then their extent being somewnat larger than that of the glass, this image is of the same figure and nearly of the same extent as the glass; but when the image is received at a great distance from the glass, where the extent of the disks is much greater than that of the glass, the image no longer retains the same figure as the glass, but becomes necessarily circular. To find the point of distance where the image loses its square figure, we have only to seek for the distance where the glass appears under an angle equal to that the sun forms to our sight, i. e. an angle of 32 minutes, and this distance will be that where the image will lose its square figure, and become round, for the disks having always an equal line to the semi-circle, which measures an angle of 32 minutes for a diameter, we shall find by this rule that a square glass of six inches loses its square figure at the distance of about 60 feet, and that a glass of a foot square loses it at 120 feet, and so on of the rest.

By reflecting a little on this theory we shall no longer be astonished to find, that at very great distances a large and small glass afford an image of nearly the same size, and which only differs by the intensity of the light; we shall no longer be surprised that a round, square, long, or triangular glass, or any other figure, always yields round images*; and we shall evidently see that images do not increase and lessen by the dispersion of light, or by any loss in passing through the air, as some naturalists have imagined; but that, on the contrary, it is occasioned by the augmentation of the disks, which always occupy a space of 32 minutes to whatever distance they are removed.

So, likewise, we shall be convinced, by the simple exposition of this theory, that curves, of any kind, cannot be used with advantage to burn at a great distance, because the diameter of the focus can never be smaller than the chord, which measures an angle of 32 minutes, and that, consequently, the most perfect concave mirror, whose diameter is equal to this chord, will never produce double the effect of a plane mirror of the same surface; and if the diameter of a curved mirror were less than the chord, it would scarcely have more effect than a plane mirror of the same surface.

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* This is the reason that the small images which pass betwixt the leaves of high and full trees, and which falling on the walks are all oval or round.

When I had well considered the above I had no longer a doubt that Archimedes could not burn at a distance but with plane mirrors, for, independently of the impossibility they then felt, and which we feel at pleasure, of making concave mirrors with so large a focus, I was well aware that the reflection I have just made could not have escaped this great mathematician. Besides, there is every reason to suppose that the ancients did not know how to make large masses of glass; that they were ignorant of the art of burning it to make large glasses, possessing only the method of blowing it, and making bottles and vases; from which consideration I was led to conclude, that it was with plane mirrors of polished metals, and by the reflections of the sun, that Archimedes had been enabled to burn at a distance. But as I

perceived

perceived that glass mirrors reflected the light more powerfully than the most polished mirrors, I thought to construct a machine to coincide in the same point the reflected images by a great number of these plane glasses, being well convinced that this was the sole mode of succeeding.

Nevertheless, I had still some doubts remaining, which appeared to me well founded, for thus I reasoned. Supposing the burning distance to be 240 feet, I perceived clearly that the focus of my mirror could not have a less than two feet diameter; in which case what would be the extent I should be obliged to give to my assemblage of plane mirrors to produce a fire in so great a focus? It might be so great that the thing would be impracticable in the execution, for, by comparing the diameter of the focus to the diameter of the mirror, in the best reflecting mirrors, I observed that the diameter of the Academy's mirror, which is three feet, was 108 times bigger than its focus, which was no more than four lines; and I concluded, that to burn as strong at 240 feet it was necessary that my assemblage of mirrors should be 216 feet diameter to have a focus of two feet; now a mirror of 216 feet diameter was certainly an impossible thing.

This mirror of three feet diameter burnt strong enough to melt gold, and I was desirou to see how much I should gain by reducing its action to the burning of wood. For this purpose I used circular zones of paper on the mirrors to diminish the diameter, and I found that there was no longer power enough to inflame dry wood when its diameter was reduced to little more than four inches; therefore, taking five inches, or sixty lines, for the diameter necessary to burn with a focus of four lines, it appeared, that to burn equally at 240 feet, where the focus should necessarily have two feet diameter, I should require a mirror of 30 feet diameter, which appeared still as impossible, or at least impracticable.

To such positive conclusions, and which others would have regarded as demonstrations of the impossibility of the mirror, I had only a supposition to oppose; but an old supposition, on which the more I reflected the more I was persuaded that it was not without foundation; namely, that the effects of heat might possibly not be in proportion to the quantity of light, or, what amounts to the same, that at an equal intensity of light large focuses must burn brisker than the small.

By estimating heat mathematically, it is not to be doubted but that the power of a focus of the same length is in proportion to the surface of the mirror. A mirror whose surface is double that of another, must have the same sized focus, and this focus must contain double the quantity of light which the first contained; and in the supposition, that effects are always in proportion to their causes, it might be presumed that the heat of this second focus should be double that of the first.

So likewise, and by the same mathematical estimation, it has always been thought, that at an equal intensity of light, a small focus ought to burn as much as a large one, and that the effect of the heat ought to be in proportion to this intensity of light: *insomuch* (says Descartes) *that glasses, or extremely small mirrors, may be made, which will burn with as much violence as the large.* I at first thought that this conclusion, drawn from mathematical theory, might be found false in practice, because heat being a physical quality, of the action and propagation of which we know not the laws, it seemed to me, that there was some kind of temerity in thus estimating its effects by a simple speculation.

I had, therefore, once more, recourse to experiments. I took metal mirrors of different focuses and different degrees of polish, and by comparing the different actions on the same fusible or combustible matters, I found, that at an equal intensity of light, large focuses constantly have more effect than small, and I discovered the same to be the case with refracting mirrors.

It is easy to assign the reason of this difference, if we consider that heat communicates nearer and nearer, and disperses, if I may so speak, when it is even applied on the same point: for example, if we let the focus of a burning glass fall on the centre of a crown piece, and that this focus was only a line in diameter, the heat produced on the centre disperses and extends over and throughout the whole piece: thus all the heat, although used at first to the centre of the crown, does not stop there, and consequently cannot produce so great an effect as if it did. But if, instead of a focus of a line which falls upon the centre of the crown, we let fall a focus of equal intensity on the whole crown, every part being alike heated, then instead of experiencing the less heat, it acquires an augmentation; for the middle profiting of the heat with the other points which surround it, the crown piece will be melted in this latter case, while in the first, it will only be slightly heated.

After these experiments and reflections, I began to entertain sanguine hopes of making mirrors to burn at a great distance; for I no longer dreaded as before, the great extent of the focus; I was persuaded, on the contrary, that a focus of a considerable breadth, as two feet, and which in the intensity of the light would not be near so great as in a small focus of four lines, might, nevertheless, produce inflammation, and with more power; and that, consequently, this mirror, which, by mathematical theory, ought to have at least thirty feet diameter, would be reduced to one of eight or ten feet at most, which was not only a possible, but even a very practicable thing.

I then thought seriously of executing my project: I had at first a design of trying to burn at 200 or 300 feet distance with circular or hexagonal glasses of a square foot in surface, and I was desirous of having four iron carriages for them, with screws to each to move them, and a spring to adjust them; but the considerable expense that this required made me quit that idea, and I took two common glasses of six inches by eight, and a wooden adjustment, which, in fact, was less solid and precise, but the expence was more consistent with a mere experiment: the mechanism of which was executed by M. Passement.

It is sufficient to say, that it was at first composed of 168 glasses of six inches by eight each, about four lines distant from each other; these glasses moved in all directions, and the four lines of space between them not only served for the freedom of this motion, but also to let the operator see the place where he was to conduct his images. By means of this construction, 168 images could be thrown on one point, and, consequently, burn at several distances, as at 20, 30, and to 150 feet. By increasing the size of the mirror, or by making other mirrors like the first, we are certain of throwing fire to still greater distances, or to increase as much as we please the force or activity of those first distances.

It is only to be observed, that the motion here spoken of is not very easy to be executed, and that also there is a very great choice to be made in the glasses; for they are not all equally good, though they appear so at the first inspection. I was obliged to pick out of more than 500 the 168 I made use of. The method of trying them is to receive at 150 feet distance the reflected image of the sun, as a vertical plane; we must select those which give a round and terminated image, and reject those, whose thicknesses being unequal in different parts, or the surface a little concave or convex, have images badly terminated, double, treble, oblong, &c. according to the different defects found in the glasses.

By the first experiment which I made the 23d of March, 1747, at noon, I set fire to a plank of fir at 66 feet distance, with 40 glasses only, about a quarter of the mirror. It must be observed that not being yet mounted, it was very disadvantageously placed, forming an angle with the sun of twenty degrees declination, and another of more than ten degrees inclination.

The same day I set fire to a pitchy and sulphureous plank at 126 feet distance, with eighty-eight glasses, the mirror being still placed disadvantageously. It is well known, that to burn with the greatest advantage the mirror should be directly opposed to the sun, as well as the matters to be inflamed; so that, by supposing a perpendicular plane on the plane of the mirror, it must pass by the sun, and, at the same time, through the midst of combustible matters.

The 3d of April, at four o'clock in the afternoon, the mirror being mounted, produced a slight inflammation on a plank covered with pitch at 138 feet distance, although the sun was weak and the light pale. Great care must be taken, when we approach the spot where the combustible matters are, not to look on the mirror; for if, unfortunately, the eyes should meet the focus, inevitable blindness will ensue.

The 4th of April, at eleven in the morning, although the sun appeared watery, and the sky cloudy, yet it produced, with 154 glasses, so considerable a heat at 158 feet, that in less than two minutes it made a deal plank smoke, and which would certainly have flamed, if the sun had not suddenly disappeared.

The ensuing day, the 5th of April, at three o'clock in the afternoon, we set fire, in a minute and a half, at 150 feet distance, to a plank sulphured and mixed with coals, with 154 glasses. When the sun is powerful, only a few seconds is required to produce inflammation.

The 10th of April in the afternoon, the sun being bright, we set fire to a fir plank at 150 feet distance, with only 128 glasses: the inflammation was very sudden, and made in all the extent of the focus, which was about sixteen inches diameter at this distance.

The same day, at half past two o'clock, we threw the fire on another plank, partly pitched and covered with sulphur in some places: the inflammation was made very suddenly; it began by the parts of the wood which were uncovered, and the fire was so violent, that the plank was obliged to be dipt in water to extinguish it: there were 148 glasses at 150 feet distance.

The eleventh of April, the focus being only 20 feet distant from the mirror, it only required 12 glasses to inflame small combustible matters; with 21 glasses we set fire to another plank which had already been partly burnt; with 45 glasses we melted a block of tin of 6lb. weight; and with 117 glasses we melted thin pieces of silver, and reddened an iron plate; and I am also persuaded, that by using all the glasses of the mirror we should have been enabled to have melted metals at 50 feet distance; and as the focus at this distance was six or seven inches broad, we should be able to make trials on all metals, which it was not possible to do with common mirrors, whose focus is either very weak or 100 times smaller than that of mine. I have remarked, that metals, and especially silver, smoke much before they melt; the smoke was so striking that it shaded the ground, and it was there I looked on it attentively, for it is not possible to look a moment on the focus when it falls on the metal,

tal, the lustre being much more dazzling than that of the sun.

The experiments which I have here related, and which were made immediately after the invention of the mirrors, have been followed by a great number of others, which confirm them. I have set fire to wood at 210 feet distance with this mirror, by the sun in summer; and I am certain, that with four similar mirrors I could burn at 400 feet, and, perhaps, at a greater distance. I have likewise melted all metals, and metallic minerals, at 25, 30, and 40 feet. We shall find, in the course of this article, the uses to which these mirrors can be applied, and the limits that must be assigned to their power for calcination, combustion, fusion, &c.*

This mirror burns according to the different inclination given it, and what gave it this advantage over the common reflecting mirrors was that its focus was very distant, and had so little curvature, that it was almost imperceptible: it was seven feet broad by eight feet high, which makes about the 150th part of the circumference of the sphere, when we burn at 150 feet distance.

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* It requires about half an hour to mount the mirror and to make all the images fall on the same point; but when this is once adjusted, it may be used at all times by simply drawing a curtain.

The reason that determined me to prefer glasses of six inches broad by eight inches high to square glasses of six or eight inches, was, that it is much more commodious to make experiments upon a horizontal and level ground than otherwise, and that with this figure, the height of which exceeded the breadth, the images were rounder; whereas with square glasses they would be shortened, especially at small distances, in a horizontal situation.

This discovery furnishes us with many useful hints for physic, and perhaps for the arts. We know that what renders common reflecting mirrors most useless for experiments is, that they burn almost always upwards, and that we are greatly embarrassed to find means to suspend or support to their focus matters to be melted or calcined. By means of my mirror we burn concave mirrors downwards, and with so great an advantage that we have what degree of heat we please; for example, by opposing to my mirror a concave one of a foot square in the surface, the heat produced to this last mirror, by using 154 glasses only, will be upwards of 12 times greater than that generally produced, and the effect will be the same as if 12 suns existed instead of one, or rather as if the sun had 12 times more heat.

Secondly, By means of my mirror we shall have the true scale of the augmentation of heat, and make a real thermometer, whose divisions will be no longer arbitrary, from the temperature of the air to what degree of heat we chuse, by letting fall, successively, the images of the sun one on the other, and by graduating the intervals, whether by means of an expansive liquor, or a machine of dilatation, and from that we shall know, in fact, what a double, treble, quadruple, &c. augmentation of heat is, and shall find out matters whose expansion, or other effects, will be the most suitable to measure the augmentations of heat.

Thirdly, We shall exactly know how many times is required for the heat of the sun to burn, melt, or calcine different matters, which was hitherto only known in a vague and very indefinite manner; and shall be in a state to make precise comparisons of the activity of our fires with that of the sun, and have exact relations and fixed and invariable measures. In short, those who examine my theory, and shall have seen the effect of my mirror, I think will be convinced the mode I have used was the only one possible to succeed to burn far off, for, independant of the physical difficulty of making large concave, spherical, parybolical mirrors, or of any other curvature whatsoever, regular enough to burn at 150 feet distance, we shall easily be convinced that they would not produce but nearly as much effect as mine, because the focus would be almost as broad; that besides, these curved mirrors, if even it should be possible to make them, would have the very great disadvantage to burn only at a nigh distance, whereas mine burns at all distances; and, consequently, we shall abandon the scheme of making mirrors to burn at a great distance by means of curves, which has uselessly employed a great number of mathematicians and artists, who were always deceived, because they considered the rays of the sun as parallel, whereas they should be considered as they are, namely, as forming angles of all sizes, from 0 to 32 minutes, which makes it impossible, whatsoever curve is given to a mirror, to render the diameter of the focus smaller than the chord, which measures 32 minutes. Thus, even if we could make a concave mirror to burn at a great distance; for example, at 150 feet, by employing all its points on a sphere of 600 feet diameter, and by employing an uncommon mass of glass or metal, it is evident that we shall have a little more advantage than by using, as I have done, only small plane mirrors.

On the whole, although this mirror is susceptible of a very great perfection, both for the adjustment, and many other particulars, and though I think I shall be able to make another, whose effects will be superior, yet, as every thing has its limits, it must not be expected that every one can be formed to burn at extreme distances; to burn, for example, at the distance of half a mile, a mirror 200 times larger would be required; and I am of opinion that more will never be effected than to burn at the distance of 8 or 900 feet. The focus, whose motion is always correspondent to that of the sun, moves so much the quicker as it is farther distant from the mirror; and at 90 feet it would move about six feet a minute.

However, as I have given an account of my discovery, and the success of my experiments, I should render to Archimedes, and the ancients, the glory that is their due. It is certain that Archimedes could perform with metal mirrors what I have done with glass, and that, consequently, I cannot refuse him the title of the first inventor of these mirrors, and the opportunity he had of using them rendered him, without doubt, more celebrated than the merit of the thing itself.

Many advantages may be derived from the use of these mirrors; by an assemblage of small mirrors, with hexagonal planes, and polished steel, which will have more solidity than glasses, and which would not be subject to the alterations which the light of the sun may cause, we may produce very useful effects, and which would amply repay the expences of the construction of the mirror.

“For all evaporations of salt waters, where great quantities of wood and coal are consumed, or structures raised for the purpose of carrying the waters off, which cost more than the construction of many mirrors, such as I mention; for the evaporation of salt waters, only an assemblage of twelve plane mirrors of a square foot each is necessary. The heat reflected by their focuses, although directed below their level, and at fifteen or sixteen feet distance, will be still great enough to boil water, and consequently produce a quick evaporation: for the heat of boiling water is only treble the heat of the sun in summer; and as the reflection of a well polished plane surface only diminishes the heat one half, only six mirrors are required to produce at the focus a heat equal to boiling water; but I shall double the number to make the heat communicate quicker; and likewise by reason of the loss occasioned by the obliquity, under which the light falls on the surface of the water to be evaporated, and because salt water heats slower than fresh. This mirror, whose assemblage would form only a square four feet broad by three high, would be easy to be managed; and if it were required to double or treble the effects in the same time, it would be better to make so many similar mirrors, than to augment the scale of them; for water can only receive a certain quantity of heat, and we should not gain any thing by increasing this degree; whereas, by making two focuses with two equal mirrors, we should double the effect of the evaporation, and treble it by three mirrors, whose focuses would fall separately one from the other on the surface of the water to be evaporated. We cannot avoid the loss caused by the obliquity; nor can it be remedied but by suffering a still greater, that is, by receiving the rays of the sun on a large glass, which would reflect them broken on the mirror; for then it would burn at bottom instead of the top, but it would lose half the heat by the first reflection, and half of the remainder by the second; so that instead of six small mirrors, it would require a dozen to obtain a heat equal to boiling water. For the evaporation to be made with more success, we ought to diminish the thickness of the water as much as possible; a mass of water a foot deep will not evaporate nearly so quick as the same mass reduced to six inches, and increased to double the superfices. Besides, the bottom being nearer the surface, it heats quicker, and this heat, which the bottom of the vessel receives, contributes still more to the celerity of the evaporation.

2. These mirrors may be used with advantage to calcine plaisters, and even calcareous stones, but they would require to be larger, and the matters placed in an elevated situation, that nothing might be lost by the obliquity of the light. It has already been observed that gypsum heats as soon again as soft calcareous stone, and nearly twice as quick as marble, or hard calcareous stone; their calcination, therefore, must be in a respective ratio. I have found by an experiment repeated three times, that very little more heat is required to calcine white gypsum, called alabaster, than to melt lead. Now the heat necessary to melt lead is, according to the experiments of Newton, eight times stronger than the heat of the summer sun; it therefore would require at least six-

teen small mirrors to calcine gypsum; and because of the losses thereby occasioned, as well by the obliquity of the light as by the inequality of the focus, which is not removed above fifteen feet, I presume it would require twenty, and perhaps twenty-four mirrors of a foot square each, to calcine gypsum in a short time, consequently it would require an assemblage of forty-eight small mirrors to calcine the softest calcareous stone, and seventy-two of a foot square to calcine hard calcareous stones. Now a mirror twelve feet broad by six feet high, would be a large and cumbersome machine; yet we might conquer these difficulties if the product of the calcination were considerable enough to surpass the expense of the consumption of wood. To ascertain this, we ought to begin by calcining plaister with a mirror of twenty-four pieces, and if that succeeded, to make two other similar mirrors, instead of making a large one of seventy-two pieces; for by coinciding the focuses of these three mirrors of twenty-four pieces, we should produce an equal heat, strong enough to calcine marble or hard stone.

But a very essential matter remains doubtful, that is, to know how much time would be requisite, for example, to calcine a cubical foot of matter, especially if that foot were struck with the heat only in one part. Some time would pass before the heat penetrated its thickness; during this time, a great part of the heat would be lost, and which would issue from this piece of matter after it had entered it. I fear, therefore, much that the stone not being touched by the heat on every side at once, the calcination would be slower, and the produce less. Experience alone can decide this, but it would be at least necessary to attempt it on gypsous matters, whose calcination is as quick again as calcareous stone.

By concentrating this heat of the sun in a kiln, which has no other opening than what admits the light, a great part of the heat would be prevented from flying off, and by mixing with calcareous stone a small quantity of coal dust, which is the cheapest of all combustible matters, this slight supply of food would suffice to feed and augment the quantity of heat, which would produce a more ample and quick calcination, and at very little expense.

3. These mirrors of Archimedes might be, in fact, used to set fire to the sails of vessels, and even to pitched wood at more than 150 feet distance; they might also be used against the enemy, by burning the grain and other productions of the earth; this effect would be

no less sudden than destructive; but we will not dwell on the means of doing mischief, conceiving it to be more our duty to think on those which may do some real service to mankind.

4. These mirrors furnish the sole means of exactly measuring heat. It is evident that two mirrors, whose luminous images unite, produce double heat in all the points of their surfaces, that three, four, five, or more mirrors, will also give a treble, quadruple, quintuple, &c. heat, and that, consequently, by this mode we can make a thermometer whose divisions will not be too arbitrary, and the scales different, like those of the present thermometers. The only arbitrary thing which would enter into the composition of the thermometer, would be the supposition of the total number of the parts of the quicksilver by quitting the degree of absolute cold; but by taking it to 10000 below the congelation of water, instead of 1000, as in our common thermometers, we should approach greatly towards reality, especially by chusing the coldest day in winter to mark the thermometers, for then every image of the sun would give it a degree of heat above the temperature of ice. The point to which the mercury rises by the first image of the sun, would be marked 1, and so on to the highest,

which might be extended to 36 degrees. At this degree we should have an augmentation of heat, thirty-six times greater than that of the first, eighteen times greater than that of the second, twelve times greater than that of the third, nine times greater than that of the fourth, and so on; this augmentation of thirty-six of heat above that of ice would be sufficient to melt lead; and there is every appearance to think that mercury, which volatilizes by a much less heat, would by its vapour break the thermometer. We cannot therefore, at most, extend the division farther than twelve, and perhaps not farther than nine degrees, if mercury be used for these thermometers, and by these means we shall have only nine degrees of the augmentation of heat. This is one of the reasons which induced Newton to make use of linseed oil instead of quicksilver; and, in fact, by making use of this liquor, we can extend the division not only to twelve degrees, but as far as to make this oil boil. I do not propose spirits of wine, because that liquor decomposes in a very short time, and cannot be used for experiments of a strong heat.*

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* Many travellers have told and written to me, that Reaumur's thermometers of spirit of wine, became quite useless to them, because this liquid lost its colour, and became charged with a sort of mud in a very short time.

When on the scale of these thermometers filled with oil or mercury, the first divisions 1, 2, 3, 4, &c. are marked to indicate the double, treble, quadruple, &c. augmentations of heat, we must search after the aliquot parts of each division; for example, of the point $1\frac{1}{4}$, $2\frac{1}{4}$, $3\frac{1}{4}$, &c. or $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, &c. and $1\frac{3}{4}$, $2\frac{3}{4}$, $3\frac{3}{4}$, and which will be obtained in an easy manner, by covering the $\frac{1}{4}$, $\frac{1}{2}$, or $\frac{3}{4}$, of the superfices of one of those small mirrors; for then the image which it reflects, will contain only the $\frac{1}{4}$, $\frac{1}{2}$, or $\frac{3}{4}$, of the heat which the whole image will contain, and, consequently, the division of the aliquot parts will be as exact as those of the whole numbers.

If once we succeed in this real thermometer, which I call real, because it actually marks the proportion of the heat, every other thermometer whose scale is arbitrary and different, will become not only superfluous, but even inimical, in many cases, to the precision of natural truths sought after by these means.

5. By means of three mirrors we may easily collect in their entire purity, the volatile parts of gold, silver, and other metals and minerals; for, by exposing to the large focus of those mirrors a large piece of metal, as a dish, or silver plate, we shall see smoke issue from it

in great abundance, and for a considerable time, till the metal is in fusion; and by giving only a smaller heat than what fusion requires, we shall evaporate the metal so as to diminish the weight considerably.

I am certain of this circumstance, which also elucidates the intimate composition of metals. I was desirous of collecting this plentiful vapour, which the pure fire of the sun causes to issue from metal, but I had not the necessary instruments, and I can only recommend to chemists and naturalists to follow this important experiment, the results of which would be as much less equivocal as the metallic vapour is pure; whereas, in all like operations made with common fire, the metallic vapour is necessarily mixed with other vapours proceeding from combustible matters, which serve for food to this fire.

Besides, this means is the only one we have to volatilize fixed metals, such as gold and silver; for I presume that this vapour, which I have seen rise in such great quantities from these fixed metals, heated in the large focus of my mirror, is neither of water, nor of any other liquor, but of the parts even of the metal which the heat detaches by volatilizing them. By receiving these vapours of different metals, and thus mixing them together, more intimate and pure alloys would be made than can be by fusion, and the mixture of these metals when melted, which never perfectly unites on account of the inequality of their specific weight, and many other circumstances which are opposed to the intimate and perfect equality of the mixture. As the constituent parts of the metallic vapours are in a much greater state of division than fusion, they would join and unite closer and more readily. In short, we should attain the knowledge of a general fact by this mode, and which, for many reasons, I have a long time suspected, that there is penetration in all alloys made in this manner, and that their specific weight would be always greater than the sum of the specific weights of the matters of which they are composed: for penetration is only a greater degree of intimacy; every thing equal in other respects will be so much the greater as matters will be in a more perfect state of division.

By reflecting on the vessels used to receive and collect these metallic vapours, I was struck with an idea, which appeared to me to be of too great utility not to publish; it is also easy enough to be realized by good able chemists; I have even communicated it to some of them, who appeared to be quite satisfied with it. This idea is to freeze mercury in this climate, and with a much less degree of cold than that of the experiments of Petersburgh or Siberia. For this purpose the vapour of mercury is only required to be received, and which is the mercury itself volatilized by a very moderate heat in a crucible, or vessel, to which we give a certain degree of artificial cold. This vapour, or this mercury, minutely divided, will offer, to the action of the cold, surfaces so large, and masses so small, that instead of 187 degrees of cold requisite to freeze mercury, possibly 18 or 20 will be sufficient, and perhaps even less to freeze it when in vapour. I recommend this important experiment to all those who endeavour earnestly for the advancement of the sciences.

To these principal uses of the mirror of Archimedes, I could add many other particular ones; but I have confined myself to those only which appeared the most useful, and the least difficult to be put in practice; nevertheless I have subjoined some experiments that I made on the transmission of light through transparent bodies, to give some new ideas on the means of seeing objects at a distance with the naked eye, or with a mirror, like that spoken of by the ancients,

cients, and by the effect of which vessels could be perceived from the port of Alexander, as far as the curvature of the earth would permit.

Naturalists at present know, that there are three causes which prevent the light from uniting in a point, when its rays have passed the objective glass of a common mirror. The first is the spherical curve of this glass, which disperses a part of the rays in a space terminated by a curve. The second is the angle under which the object appears to the naked eye: for the breadth of the focus of the objective glass has a diameter nearly equal to the chord of which this angle measures. The third is the different refrangibility of the light; for the most refrangible rays do not collect in the same place with the lesser.

The first cause may be remedied by substituting, as Descartes has proposed, elliptical, or hyperbolical, glasses to the spherical. The second is to be remedied by a second glass, placed to the focus of the objective, whose diameter is nearly equal the breadth of this focus, and whose surface is worked on a sphere of a very short ray. The third has been found to be remedied, by making telescopes, called Acromatics, which are composed of two sorts of glasses, which disperse the coloured rays differently; so that the dispersion of the one is corrected by the other, without the general refraction, which constitutes the mirror, being destroyed. A telescope 3½ feet long, made on this principle, is in effect equivalent to the old telescopes of 25 feet.

But the remedy of the first cause is perfectly useless at this time, because the effect of the last being much more considerable, has such great influence on the whole effect, that nothing can be gained by substituting hyperbolical, or elliptical glasses to spherical, and this substitution could not become advantageous, but in the case where the means of correcting the effect of the different refrangibility of the rays of light might be found; it seems, therefore, that we should do well to combine the two means, and to substitute, in acromatic telescopes, elliptical glasses.

To render this more obvious, let us suppose the object observed to be a luminous point without extent, as a fixed star is to us. It is certain, that with an objective glass, for example, of 30 feet focus, all the images of this luminous point will extend in the form of a curve to this focus, if it be worked on a sphere; and, on the contrary, they will unite in one point if this glass be hyperbolical; but if the object observed have a certain extent, as the moon, which occupies half a degree of space to our eyes, then the image of this object will occupy a space of three inches diameter in the focus of the objective glass of thirty feet; and the aberration caused by the sphericity producing a confusion in any luminous point, it produces the same on every luminous point of the moon's disk, and, consequently, wholly disfigures it. There would be, then, much disadvantage in making use of elliptical glasses or long telescopes, since the means have been found, in a great measure, to correct the effect produced by the different refrangibility of the rays of light.

From this it follows, that if we would make a telescope of 30 feet, to observe the moon, and see it completely, the ocular glass must be at least three inches diameter, to collect the whole image which the objective glass produces to its focus; and if we would observe this planet with a telescope of 60 feet, the ocular glass must be at least six inches diameter, because the chord which the angle measures under which the moon appears to us, is, in this case, nearly six inches; therefore astronomers never make use of telescopes that include the whole disk of the moon, because they would magnify but very little. But if we would observe the planet Venus with a telescope of 60 feet, as the angle under which it appears to us is only 60 seconds, the ocular glass can only have four lines diameter; and if we make use of an objective of 120 feet, an ocular glass of eight lines diameter would suffice to unite the whole image which the objective forms to its focus.

Hence we see, that even if the rays of light were equally refrangible we could not make such strong telescopes to see the moon with as to see the other planets, and that the smaller a planet appears to our sight the more we can augment the length of the telescope, with which we can see it wholly. Hence it may be well conceived, that in this supposition of the rays, equally refrangible, there must be a certain length more advantageously determined than any other for each different planet, and that this length of the telescope depends not only on the angle under which the planet appears to our sight, but also on the quantity of light with which it is brightened.

In common telescopes the rays of light being differently refrangible, all that could be done in this mode to give them perfection would be of very little advantage, because, that under whatever angle the object, or planet, appears to our sight, and whatever intensity of light it may have, the rays will never collect in the same part; the longer the telescope the more interval it will have between the focus of the red and violet rays, and consequently the more confused the image of the object observed.

Refracting telescopes, therefore, can be rendered perfect only by seeking for the means of correcting this effect of the different refrangibility, either by composing telescopes of different densities, or by other particular means, which would be different according to different objects and circumstances. Suppose, for example, a short telescope, composed of two glasses, one convex and the other concave; it is certain that this telescope might be reduced to another whose two glasses would be plain on one side, and on the other bordering on spheres, whose rays would be shorter than that on the spheres on which the glasses of the first telescopes have been constructed. However, to avoid a great part of the effect of the different refrangibility of the rays, the second telescope may be made with one single piece of massive glass, as I had it done with two pieces of white glass, one of two inches and a half in length, and the other one inch and a half; but then the loss of transparency is a greater inconvenience than the different refrangibility which it corrects, for these two small massive telescopes of glass are more obscure than a small common telescope of the same glass and dimensions; they indeed give less iris, but are not better; for in massive glass the light, after having crossed this thickness of glass, would no longer have a sufficient force to take in the image of the object to our eye. So to make telescopes 10 or 20 feet long, I find nothing but water that has sufficient transparency to suffer the light to pass through this great thickness. By using, therefore, water to fill up the intervals between the objective and the ocular glass, we should in part diminish the effect of the different refrangibility, because water approaches nearer to glass than air, and if we could, by loading the water with different salts, give it the same refringent degree of power as glass, it is not to be doubted, that we should correct still more, by this means, the different refrangibility of the rays. A transparent liquor should, therefore, be used, which would have nearly the same refrangible power as glass, for then it would be certain that the two glasses, with their liquor between them, would in part correct the effect of the different refrangibility of the rays, in the same mode as it is corrected in the small massive telescope which I speak of.

According to the experiments of M. Bouguer, the thickness of a line of glass destroys $\frac{2}{7}$ of light, and consequently the diminution would be made in the following proportion:

Thickness, 1, 2, 3, 4, 5, 6 lines
Diminution, $\frac{2}{7}$ $\frac{10}{49}$ $\frac{50}{343}$ $\frac{250}{2401}$ $\frac{1250}{16807}$ $\frac{6250}{117694}$:

So that by the sum of these six terms we should find, that the light which passes through six lines of glass would lose $\frac{102024}{117649}$, that is, about $\frac{10}{11}$ of its quantity. But it must be considered, that M. Bouguer makes use of glasses which are but little transparent, since he has observed, that the thickness of a line of these glasses destroys $\frac{2}{7}$ of the light. By the experiments which I have made on different kinds of white glass, it has appeared to me that the light diminishes much less. These experiments are easy to be made, and are what all the world may repeat.

In a dark chamber, whose walls were blackened, and which I made use of for optical experiments, I had a candle lighted of five to the pound; the room was very large and the candle the only light in it; I then tried at what distance I could read by this light, and found that I read very easily at 24 feet four inches from

from the candle. Afterwards, having placed a piece of glass, about a line thick, before it, at two inches distance, I found that I still read very plainly at 22 feet nine inches; and substituting to this glass another piece of two lines in thickness and of the same glass, I read at 21 feet distance from the candle. Two of the same glasses joined one to the other, and placed before the candle diminished the light so much that I could only read at $17\frac{1}{2}$ feet distance; and at length, with three glasses, I could only read at 15 feet. Now the light of a candle diminishing as the square of the distance augments, its diminution should have been in the following progression, if glasses had not been interposed: 2—$24\frac{1}{3}$. 2—$22\frac{3}{4}$ 2—21. 2—$17\frac{1}{2}$. 2—15, or $592\frac{1}{9}$. $517\frac{9}{16}$ 441. $306\frac{1}{4}$. 225. Therefore the loss of the light, by the interposition of the glasses, is in the following progression: $84\frac{79}{144}$. 151. $285\frac{7}{9}$. $367\frac{1}{4}$.

From hence it may be concluded, that the thickness of a line of this glass diminishes only $\frac{84}{592}$ of light, or about $\frac{1}{7}$; that two lines diminishes $\frac{157}{592}$, not quite $\frac{1}{4}$ and three glasses of two lines $\frac{397}{592}$, i. e. less than $\frac{2}{3}$.

As this result is very different from that of M. Bouguer, and as I was cautious of suspecting the truth of his experiments, I repeated mine with common glass. For long telescopes water alone can be used; and it is still to be feared that an inconveniency will subsist, from the opacity resulting from the quantity of liquor which fills the interval between the two glasses.

The longer the telescope the greater loss of light will ensue; so that it appears at first sight that this mode cannot be used, especially for long telescopes; for following what M. Bouguer says in his Optical Essay, cn the gradation of light, nine feet seven inches sea-water diminishes the light in a relation of 14 to 5; therefore these long telescopes, filled with water, cannot be used for observing the sun, and the stars would not have light enough to be perceived across a thickness of 20 or 30 feet of intermediate liquor.

Nevertheless, if we consider, that by allowing only an inch, or an inch and a half, for the bore of an objective of 30 feet, we shall very distinctly perceive the planets in the common telescopes of this length; we may suppose that by allowing a greater diameter to the objective we should augment the quantity of light in the ratio of the square of this diameter, and, consequently, if an inch before suffices to see a star distinctly, in a common telescope, three inches bore would be sufficient to see it distinctly through a thickness of 10 feet water, and that with a glass of three inches diameter we should easily see it through a thickness of 20 feet water, and so on. It appears, therefore, that we might hope to meet with success in constructing a telescope on these principles; for, by increasing the diameter of the objective, we partly regain the light lost by the defect of the transparency of the liquor.

But it appears to me certain that a telescope constructed on this mode would be very useful for observing the sun; for supposing it even the length of 100 feet, the light of that luminary would not be too strong after having traversed this thickness of water, and we should be enabled to observe its surface easily, and at leisure, without the need of making use of smoked glasses, or of receiving the image on pasteboard; an advantage we cannot possibly derive from any other telescope.

There would require only some trifling difference in the construction of this solar telescope, if we wanted the whole face of the sun presented; for supposing it the length of 100 feet, in this case, the ocular glass must be ten inches diameter; because the sun, taking up more than half a celestial degree, the image formed by the objective to its focus at 100 feet, will at least have this length of ten inches; and to unite it wholly, it will require an ocular glass of this breadth, to which only twenty inches of focus should be given to render it as strong as possible. It is necessary that the objective, as well as the ocular glass, should be ten inches in diameter, in order that the image of the sun, and the image of the bore of the telescope, be of an equal size with the focus.

If this telescope, which I propose, should only serve to observe the sun exactly, it would be of great service; for example, it would be very curious to be able to discover whether there be any luminous parts larger than others in the sun; if there be inequalities on its surface; and of what kind; if the spots float on its surface; or whether they be fixed there, &c. The brightness of its light prevents us from observing this luminary with the naked eye, and the different refrangibility of its rays, renders its image confused when received in the focus of an objective glass, or on pasteboard, so that the surface of the sun is less known to us than that of any of the planets. The different refrangibility of its rays would be but little corrected in this long telescope filled with water; but if the liquor could, by the addition of salts, be rendered as dense as glass, it would then be the same as if there were only one glass to pass through; and it appears to me that infinitely more advantage would result from making use of these telescopes filled with water, than from the common telescopes with smoked glasses.

Whether that would or would not be the fact, this however is certain, that to observe the sun, a telescope quite different is required from those that we make use of for the different planets; and it is also certain, that a particular telescope is necessary for each planet, proportionate to their intensity of light, that is, to the real quantity of light with which they appear to be enlightened. In all telescopes the objectives are required as large, and the ocular glass as strong, as possible, and, at the same time, the distance of the focus proportioned to the intensity of the light of each planet. To do this with the greatest advantage, it is requisite to use only an objective glass so much the larger, and a focus so much the shorter, according to the light of the planet. Why has there not hitherto been made objective glasses of 243 feet diameter? The aberration of the rays, occasioned by the sphericity of the glasses, is the sole cause of the confusion, which is as the square of the diameter of the tube; and it is for this reason that spherical glasses, with a small bore, are of no value when enlarged; we have more light, but less distinction and clearness. Nevertheless, broad spherical glasses are very good for night telescopes. The English have constructed telescopes of this nature, and they make use of them very advantageously to see vessels at a great distance in dark nights But at present, that we know, in a great measure, how to correct the effects of the different refrangibility of the rays, it seems, that we should make elliptical or hyperbolical glasses, which would not produce the alteration caused by sphericity, and which, consequently, would be three or four times broader than spherical glasses. There is only this mode of augmenting to our sight the quantity of light sent to us from the planets, for we cannot put an additional light on them, as we do on objects which we observe with the microscope, but must at least employ to the greatest possible advantage, the quantity of light with which they are illumined, by receiving it on as great a surface as possible. This hyperbolical telescope, which would be composed only of one single large objective glass, and of an ocular one proportionate, would require matter of the greatest transparency; and we should unite by this means all the advantages possible, that is, those of the acromatic to that of the elliptical or hyperbolical telescopes, and we should profit by all the quantity of light each planet reflects to our sight. I may be deceived; but what I propose appears to be sufficiently founded to recommend its execution to persons zealously attached to the advancement of the sciences.

Employing myself thus on these reveries, some of which may one day be realized, and in which hope I publish them, I thought of the Alexandrian mirror, spoken of by some ancient authors, and by means of which vessels were seen at a great distance on the sea. The most positive passage which I have met with is the following.

"Alexandria .... in Pharo vero erat specu-
"lum e ferro sinico. Per quod a longe vide-
"bantur naves Græcorum advenientes; sed
"paulo postquam Islamismus invaluit, scilicet
"tempore califatus Walidfil: Abdi-I-melec,
"Christiani, fraude adhibita illud deleverunt.
"Abu-l-feda, &c. Descriptio Ægypti."

Having dwelt for some time on this, I have thought, 1. That such a mirror was possible to be made. 2. That even without a mirror or telescope, we might by certain

dispostions

dispositions obtain the same effect, and see vessels from land, as far, perhaps, as the curvature of the earth would permit. We have already observed that persons whose sight was very good, have perceived objects illumined by the sun at more than 3400 times their diameter, and at the same time we have remarked, that the intermediate light was of such great hurt to that of distant objects, that by night a luminous object is perceived at ten, twenty, and perhaps a hundred times greater distance than during the day. We know that at the bottom of very deep pits, stars may be seen in the daytime*; why therefore should we not see vessels illumined by the rays of the sun, by placing one's self at the end of a very long dark gallery, situated on the sea shore, in such a manner as to receive no other than that of the distant sea, and the vessels which might be on it? This gallery would be only a horizontal pit, which would have the same effect with respect to ships as the vertical pit has with respect to the stars; and it appears to me so simple, that I am astonished it has never before been thought of and tried. It seems to me, that by taking the time of the day for our observations

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* Aristotle is, I believe, the first that ever mentioned this observation.

observations when the sun should be behind the gallery, we might see them from the dark end of it ten times at least better than in the open light. Now a man on horseback is easily distinguished at a mile distance, when the rays of the sun shine on him, and by suppressing the intermediate light which surrounds us, and darkening our sight, we should see him at least ten times farther; that is to say, ten miles. Ships, therefore, being much larger, would be seen as far as the curvature of the earth would permit, without any other instrument than the naked eye.

But a concave mirror, of a great diameter, and of any focus, placed at the end of a long black tube, would have nearly the same effect as our great objective glasses of the same diameter and form would have during the night, and it was probably one of these concave mirrors of polished steel that was established at the port of Alexandria*. If this steel mirror did really exist, we cannot refuse to the ancients the glory of the first invention, for this mirror can only be effective by as much as the light reflected by its surface was collected by another concave mirror placed at its focus, and in this consists the essence of the telescope and the merit of its construction. Nevertheless this does not deprive the great Newton of any glory, who first renewed the almost-forgotten invention. As the rays of light are by their nature differently refrangible, he was inclined to think there were no means of correcting this effect, or, if he had perceived those means, he judged them so difficult that he chose rather to turn his views another way, and produce, by means of the reflection of the rays, the great effects which he could not obtain by their refraction; he, therefore, constructed his telescope, the reflection of which is infinitely superior to those that were in common use. The best telescopes are always dark in comparison of the acromatic, and this obscurity does not proceed only from the defect of the polish, or the colour of the metal of mirrors, but from the nature even of light, the rays of which being differently refrangible are also differently reflexible, although in much less unequal degrees.

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* From time immemorial the Chinese, and particularly the Japanese, have possessed the art of working in steel, both in large and small bodies; and hence I have thought that the words *e ferro sinico* in the preceding quotation should be understood as applying to polished steel.

It still remains, therefore, to bring the telescope to perfection, and to find the manner of compensating this different reflexibility, as we have discovered that of compensating the different refrangibility.

After all, I imagine that it will be well perceived that a very good day-glass may be made, without using either glasses or mirrors, and simply by suppressing the surrounding light, by means of a tube 150 or 250 feet long, and by placing ourselves in an obscure place. The brighter the day is, the greater will be the effect. I am persuaded that we should be able to see at 15, and perhaps 20 miles distance. The only difference between this long tube, and the dark gallery, which I have spoken of, is, that the field, or the space seen, would be smaller, and precisely in the ratio of the square of the bore of the tube to that of the gallery.

OBSERVATIONS AND EXPERIMENTS ON TREES AND OTHER VEGETABLES.

THE physical study of Vegetables is one of those sciences which require a multiplicity of observations and experiments beyond the capacity of one man, and must consequently be a work of time; even the observations themselves are seldom of much value till they have been repeatedly made, and compared in different places and seasons, and by different persons of similar ideas. It was for this purpose that Buffon united with M. Du Hamel, to labour, in concert for the illustration of a number of phenomena, which appeared difficult to explain, in the vegetable kingdom, and from the knowledge of which may result an infinity of useful matters in the practice of agriculture.

The frost is sometimes so intense during winter, that it destroys almost all vegetables, and the scarcity in the year 1709 was a melancholy proof of its cruel effects. Seeds, and some kinds of trees, entirely perished, while others, as olives, and almost all fruit-trees, shared a milder fate, shooting forth their leaves, their roots not having been hurt; and many large trees, which were more vigorous, shot forth every branch in spring, and did not appear to have suffered any material injury. We shall, nevertheless, remark on the real and irreparable damage this winter occasioned them.

Frost, which can deprive us of the most necessary articles of life, destroys many kinds of useful trees, and which scarcely ever leaves one insensible of its rigour, is certainly one of the most formidable misfortunes of human nature; we have therefore every reason to dread intense frosts, which might reduce us to the last extremities if their severities were frequent; but fortunately we can quote only two or three winters which have produced so great and general a calamity as that in 1709.

The greatest spring frosts, although they damage the grain, and principally barley, when it is but just eared, never occasion great scarcities. They do not affect the trunks or branches of trees, but they totally destroy their productions, deprive us of the harvest of the vines and orchards, and by the suppression of new buds cause a considerable damage to forests.

Although there are some examples of winter frosts having reduced us to a scarcity of bread, and deprived us of vegetables, the damage which spring frosts occasion becomes still more important, because they afflict us more frequently, and their effects are felt almost every year.

To consider frost even very superficially, we must perceive that the effects produced by the sharp frosts of winter are very different from what are occasioned by those in spring, since the one attacks the body and most solid parts of trees, whereas the other simply destroys their productions, and opposes their growth; at the same time they act under quite different circumstances; and it is not always the ground in which the winter frosts produce the greatest disorders, as that generally suffers most from those in the spring frosts.

It was from a great number of observations that we have been able to make this distinction on the effects of frost, and which we hope will not be simply curious, but prove of utility, and be profitable to agriculture; and should they not wholly enable us to escape from the evils occasioned by frost, they will afford us a means to guard against them. We shall, therefore, enter upon the detail, beginning with that which regards the sharp frosts of winter: of these, however, we cannot reason with so great a certainty as on those of spring, because, as we have already observed, we are seldom subjected to their tragical effects.

Most trees during winter being deprived of blossoms, fruits, and leaves, have generally their buds hardened so as to be capable of supporting very sharp frosts, unless the preceding summer was cool, in which case the buds not being arrived to that degree of maturity, which gardeners call *aoutes**, they are not in a state of resisting the moderate frosts of winter; but this seldom happens, the buds commonly ripening before winter, and the trees endure the rigour of that season without being damaged, unless excessive cold weather ensue, joined to the circumstances hereafter mentioned.

We have, nevertheless, met with many trees in forests with considerable defects, which have certainly been produced by the sharp frosts, and which will never be effaced.

These defects are, 1st, chaps or chinks, which follow the direction of the fibres. 2. A portion of dead wood included in the good; and lastly, the double sap, which is an entire crown of imperfect wood. We must dwell a little a little on these defects to trace the causes whence they proceed.

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* Ripened or filled with sap.

The sappy part of trees is, as is well known, a crown or circle of white or imperfect wood of a greater or less thickness, and which in almost all trees is easily distinguished from the sound wood, called the *heart*, by the difference of its colour and hardness; it is found immediately under the bark, and surrounds the perfect wood, which in sound trees is nearly of the same colour, from the circumference to the centre. But in those we now speak of, the perfect wood was separated by another circle of white wood, so that on cutting the trunks of them we saw alternately circles of sap and perfect wood, and afterwards a clump of the latter, which was more or less considerable, according to the different soils and situations; in strong and forest earth it is more scarce than in glades and light earth.

By the mere inspection of these cinctures of white wood, which we in future shall term *false sap*, we could perceive it to be of bad quality; nevertheless, to be certain of it, we had several planks sawed two feet in length, by nine to ten inches square, and having the like made from the true sap, we had both loaded in the middle, and those of the false sap

always

always broke under a less weight than those of the true, though the strength of the true sap is very trivial in comparison with that of formed wood.

We afterwards took several pieces of these two kinds of sap, and weighed them both in the air and water, by which we discovered that the specific weight of the natural sap was always greater than that of the false. We then made a like experiment with the wood of the centre of the same trees, to compare it with that of the cincture which is found between these two saps, and we discovered that the difference was nearly the same as is usual between the weight of the wood of the centre of all trees and that of the circumference; thus all that is become perfect wood in these defective trees is found nearly in the common order. But it is not the same with respect to the false sap, for, as these experiments prove, it is weaker, softer, and lighter than the true sap, although formed 20, nay 25 years before, which we discovered to be the fact, by counting the annual circles, as well of the sap as of the wood which covered it; and this observation, which we have repeated on a number of trees, incontestibly proves that these defects had been caused by the hard frost of 1709, notwithstanding

standing that the number of some of their coats was less than the years which had passed since that period; and at which we must not be surprised, not only because we can never, by the number of ligneous coats, find the age of trees within three or four years, but also because the first ligneous coats, formed after that frost, were so thin and confined, that we cannot very exactly distinguish them.

It is also certain, that it was the portion of the trees that were in sap in the hard frost of 1709, which instead of coming to perfection, and converting itself into wood, became more faulty. Besides, it is more natural to suppose, that the faulty part must suffer more from sharp frosts than sound wood: because it is not only at the external part of the tree, and therefore more exposed to the weather, but also because the fibres are more tender and delicate than the wood. All this at first appears to wear but little difficulty, yet the objections related in the history of the Academy of Sciences, 1710, might be here adduced; by these objections it appears that in 1709, the young trees endured the hard frost much better than old. But as these facts are certain, there must be some difference between the organic parts, the vessels, the fibres, &c. of the sappy part of the old trees and that of the young; they perhaps will be more supple, so that a power which will be capable of causing the one to break, will only dilate the other.

But as these are conjectures with which the mind remains but little satisfied, we shall pass slightly over them, and content ourselves with the particulars we have well observed. That this sappy part suffered greatly from the frost is an incontestible fact, but has it been entirely disorganized? This might happen without the death of the tree ensuing, provided the bark remained sound; and even vegetation might continue. Willows and limes frequently subsist only by their bark, and the same thing has been seen at the nursery of Roule in an orange tree. But we do not think that the false sap is dead, because it always appeared to us in quite a different state from the sap found in trees, which had a portion of dead wood included in the sound; besides, if it had been disorganized, as it extends over the whole circumference, it would have interrupted the lateral motion of the sap, and the wood of the centre, not being able to vegetate, would have also perished and altered, which was not the case, and which I could confirm by a number of experiments; however, it is not easily conceivable how this sappy part of wood has been changed so far as not to become wood, and that far from being dead, it was even in a state of supplying the ligneous coats with sap, which are formed from above in a state of perfection, and which may be compared to the wood of trees that have suffered no accident. This must nevertheless have been done by the hard winter, which caused an incurable malady to this part of the tree; for if it were dead, as well as the bark which cloathed it, there can be no doubt that the tree would have entirely perished, which happened in 1709 to many trees whose bark was detached from them, and which by the remaining sap in their trunk, shot forth their buds in spring, but died through weakness before autumn, for want of receiving sufficient nutriment to subsist on.

We have met with some of these false sappy part of trees which are thicker on one side than the other, and which surprisingly agrees with the most general state of the sap. We have also seen others very thin, so that apparently there were only the outer coats injured. These were not all of the same colour, had not undergone an equal alteration, nor were equally affected, which agrees with what we have before advanced. At length, we dug at the foot of some of these trees, to see if the defect existed also in the roots, but we found them sound: therefore, it is probable that the earth which covered them had repaired the injury done by the frost.

Here then we see one of the most dreadful effects of winter frosts, which though locked up within the tree, is not less to be feared, since it renders the trees attacked by them almost useless; but besides this, it is very difficult to meet with trees totally exempt from these injuries; and indeed all those whose wood is not of a deeper colour at the centre, growing somewhat lighter towards the sap, may be suspected of having some defects, and ought not to be made use of in any matter of consequence.

By horizontally sawing the bottom of trees, we sometimes perceive a piece of dead sap or dried bark, entirely covered by the live wood: this dead sap occupies nearly half of the circumference in the parts of the trunk where it is found: it is sometimes browner than good wood, and at others almost white. From the depth also where this sap is found in the trunk, it appears to have been occasioned by the sharp frost in winter, by which a portion of the sap and bark perished, and was afterwards covered by the new wood; for this sap is almost always found exposed to the south, where the sun melting the ice, a humidity results, which again freezes soon after the sun disappears, and that forms a true ice, which is well known to cause a considerable prejudice to trees. This defect does not always appear throughout the whole length of the trunk, for we have seen many square pieces which seemed perfectly exempt from all defects, nor were the injuries of the frost discovered until they were slit into planks. It is, nevertheless easily to be conceived, how such a disorder, in their internal parts, must diminish their strength, and assist their perishing.

In forests, or woods, we meet with trees which strong winter frosts have split according to the direction of their fibres; these are marked with a ridge formed by the cicatrice that covers the cracks, but which remain within the trees without uniting again, because a re-union is never formed in the ligneous fibres when they have been divided or broken; nor can it be doubted, that the sap, which increases in volume when it freezes, as all liquors do, may produce many of these cracks. But we also suppose that there are some which are independent of the frost, and which have been occasioned by a too great abundance of sap.

Be this as it may, the fact is, we have found defects of this kind in all soils, and in all expositions, but most frequently in wet ground and in northern and western expositions; the latter may perhaps proceed in cases when the cold is more intense, in such expositions; and in the other, from the trees which are in marshy grounds, having the tissue of their ligneous fibres weaker, and because their sap is more abundant and aqueous than in dry land; which may be the cause that the effect of the rarefaction of liquors by the pores is more perceptible, and more in a state of diminishing the ligneous fibres, as they bring less resistance thereto.

This reasoning seems to be confirmed by another observation; namely, that resinous trees, as the fir, are seldom injured by the sharp frosts of winter, evidently from their sap being more resinous: for we know that oils do not perfectly freeze, and that instead of augmenting in volume, like water, in frosty weather, they diminish when they congeal.

Dr. Hales says in his *Vegetable Statics*, *p*. 16, that the plants which transpire the least, are those which best resist the winter; because they have need of only a small quantity of nutriment to preserve themselves. He says, likewise in the same part, that the plants, which preserve their leaves during winter, are those which transpire the least; nevertheless, we know that the orange tree, the myrtle, and still more the jessamine of Arabia, &c. are very sensible to frost, although these trees preserve their leaves during winter; we must, therefore, have recourse to another cause to explain why certain trees which do not shed their leaves in winter, so well support the sharpest frosts.

We have sawed many trees which were attacked with this malady, and have almost always found, under the prominent cicatrice, a deposit of sap or rotten wood, and they are easily distinguished from what are called in the forest terms, sinks or gutters, because the defects which proceed from an alteration of the ligneous fibres, which is internally produced, occasion no cicatrice to change the external form of the trees, whereas the chinks produced by frosts, which proceed from a cleft afterwards covered by a cicatrice, make a ridge or eminence in the form of a cord, which announces the internal defect.

The sharp winter frosts produce, without doubt, many other injuries to trees, and we have remarked many defects, which we might attribute to them with great probability; but, as we have not been able to verify the fact, we shall pass on to the effects of the advantages and disadvantages of different expositions with respect to frost; for this question is too interesting to agriculture not to attempt its elucidation, especially as various authors have supported an opposition of sentiment more capable of breeding doubts than increasing our knowledge. Some have insisted that the frost is felt more strongly at the northern exposition, while others assert it is more sensible to the south or west, and all these opinions are founded on a single observation. We nevertheless perceive what has caused this diversity of opinion, and we are therefore enabled to reconcile them. But, before we relate the observations and experiments which have led us thereto, it is but just we should give a more exact idea of the question.

It is not doubted that the greatest cold proceeds from the north, for that is in the shade of the sun, which alone, in sharp frosts, tempers the rigour of the cold; besides, a situation to the north, is exposed to the north-east, and north-west winds, which are clearly the most intense, whether we judge from the effects which those winds produce, or from the liquor of the thermometers, whose decision is much more certain. It may also be observed along the espaliers, that the earth is often frozen and hardened all the day towards the north, while it may be worked upon towards the south. Moreover when a strong frost succeeds in the night, it is evident, that it must be much colder in the part where it is already formed, than in that where the earth is warmed by the sun ; this is also the reason why, even in hot countries, we find snow in the northern exposition, on the back of lofty mountains : besides, the liquor of the thermometer is always lower at the northern exposition, than in that of the south ; therefore, it is incontestible, that it is colder there, and freezes stronger.

It is therefore certain, that all the accidents which depend solely on the power of the frost, will be found more frequently at the northern exposition than elsewhere. But yet it is not always the great power of the frost which injures trees, for there are particular accidents, which cause a moderate frost to do them more prejudice than the much sharper, when they happen in favourable circumstances. Of this we have already given an example in speaking of that part of dead wood included in the good, which is produced by the hoar frost, and is found most frequently in the expositions to the south ; and it is also to be observed, that great part of the disorders produced in the winter of 1709, are to be attributed to a false thaw, which was followed by a frost still sharper than what had preceded; but the observations which we have made on the effects of spring frosts supply us with many similar examples, which incontestibly prove it is not in the expositions where it freezes the strongest, that the frost commits the greatest injuries to vegetables. Not to dwell upon assertions, we shall proceed to a detail of facts, which will render these general positions clear and apparent.

In the winter 1734 we caused a coppice in my wood, near Montbard in Burgundy, to be cut, which measured one hundred and fifty-four feet, situated in a dry place, on a flat ground, surrounded on all sides with cultivated land. In this wood we left many small square pieces without felling them, and in a manner that each equally faced east, west, north and south. After having well cleared the part that was cut, we observed carefully in spring the growth of the young buds; the renewed tops on the 20th of April, had sensibly shot out in the parts exposed to the south, and which consequently were sheltered from the north by the tufted tops; these were the first buds that appeared, and were the most vigorous; those exposed to the east appeared next; then those of the west, and lastly those of the northern exposition. On the 28th of April the frost was very sharp in the morning accompanied by a north wind; the sky was clear, and the air very dry, and in which manner it continued for three days. At the end of which I went to see in what state the buds were about the clumps, and found them absolutely blackened in all the parts exposed to the south and sheltered from the north wind, whereas those which were exposed to the cold north wind, which still blowed, were only slightly injured; and with respect to the eastern and western expositions, they were that day nearly alike injured.

The 14th, 15th, and 22d of May, it froze pretty sharply, accompanied by the north and north-west winds, and I then likewise observed that all those sheltered from the wind were very much injured, but that all those which were exposed thereto had suffered but very little. This experiment appeared decisive, and showed that although it froze most strong in parts exposed to the north wind, yet the frost in that situation did the least injury to vegetables.

This circumstance is certainly opposed to common prejudice; but it is not less the fact, and is even easy to be explained; for this purpose, it is sufficient to pay attention to circumstances in which frost acts, and we shall discover that humidity is the principal cause of its effects, so that all which occasions humidity renders, at the same time, the frost dangerous to vegetables, and all that dissipates humidity, even if it should be done by increasing the cold (for every thing that dries diminishes the disasters of a frost) acts towards their preservation.

We have often remarked, that in low places, where mists and fogs reign, frost is felt more sharply, and oftener than elsewhere. For instance, in autumn and spring we have seen delicate plants frozen in a kitchen-garden, in a low situation, while the like plants were preserved sound in another kitchen-garden situated on an eminence. So, likewise, in vallies and low forests the wood is never of a beautiful vein, nor of good quality, although the vallies are often by much the best soil. The coppice wood is never good in low places, although it shoots forth there later than upon high places, and which is occasioned by a freshness that is always concentered therein. When I walked at night in the wood I felt almost as much heat on eminences as in the open plains, but in the vallies I experienced a sharp and uncomfortable cold. Though the trees shoot out the latest in those parts, yet the shoots are still injured by the frost, which spoiling the principal buds obliges the trees to shoot forth lateral branches, and thus prevents their ever becoming straight and handsome trees fit for service. What we have just advanced must not be understood only of deep vallies, which are liable to those inconveniencies from northern expositions, or those inclosed on the southern side in the form of an alley, in which it often freezes the whole year, but also of the smallest vallies, so that by a little custom we can discover the bad figure of the shoots from the inclination of the earth; this I particularly observed on the 28th of April, 1734; on that day the buds of all the trees, from one year up to six or seven, were frozen in all the lower places; whereas in the high and uncovered places there were only the shoots near the earth which were so; the earth was then very dry, and the humidity of the air did not appear to have greatly contributed to this injury. Neither vines, nor the trees of the plain, are subject to frost, which might lead us to suppose they are less delicate than the oak; but we think this must be attributed to the humidity, which is always greater in the woods than in the rest of the plains, for we have observed that oaks are often very much injured from frosts in forests, while those which are in the plains are not hurt in the least.

Large timbers, even on eminences, may cause the young trees near them to be in the same state as if at the bottom of a valley. We have also remarked, that the young wood near large trees is often more injured by the frost than in parts remote from them, as in the midst of such woods, where a great number of branches are left, it is felt with more force than in those which are open. Now all these disorders are most considerable in such places, for as the wind and sun cannot dissipate the transpiration of the earth and plants, there remains a considerable humidity, which causes a very great prejudice to plants.

We have also remarked, that the frost is never more to be dreaded, with respect to the vine flowers, buds of trees, &c. than when it succeeds mists, or even rain, however slight, for they are all capable of enduring a very considerable degree of cold without being damaged, when it has not rained for some time, and the earth is dry.

Frosts likewise act more powerfully in places newly cultivated than in others, because the vapours, which continually rise from the earth, transpire more freely and abundantly from that which is newly cultivated. To this reason we must, however, subjoin the fact, that plants, newly set, shoot forth more vigorously than others, which renders them more sensible and liable to the effects of frost. So also in light and sandy soil the frost does more injury than in strong land, even though of equal dryness, because more exhalations escape from the first kind of earths than from the latter; and if a vine newly dunged is most subject to the frost, it arises from the humidity which escapes from it. A furrow of vine which lies along a field of sainfoin, peas, &c. is often all destroyed by the frost, while the rest of the vine is quite healthy, and this is undoubtedly, to be attributed to the transpiration of the sainfoin, or other plants, which bring a humidity on the shoots of the vine. In the vine also, the branches that are strong and cut are always less injured than the stock; especially when not attached to the props, as they are then agitated by the wind which dries them.

The same thing is remarked of timber, and I have seen in copses all the buds entirely destroyed by the frost, while the upper shoots had not received the least damage; indeed it always appeared that the frost did most injury nearest to the earth, commonly within one or two feet, insomuch that it must be very violent to destroy the buds higher than four.

All these observations, which may be regarded as very constant, agree to prove that in general it is not the sharpest frosts which do the greatest injury to plants, but that they are affected in proportion as they are loaded with humidity, which perfectly explains why the frost causes so many disorders in the southern exposition, although it should be less cold than that of the north, and likewise why the frost causes more injury to the northern exposition, when after a rain proceeding from a westerly wind the wind veers to the north towards sunset, as often happens in spring, or when, by an easterly wind, a cold moist air arises before sun-rise, which, however, is not so common.

There are likewise circumstances where the frost does most injury to the eastern exposition; but as we have many observations on that subject, we shall first relate those which we made in the spring frost in 1736, which occasioned so much damage. It having been very dry previously, it froze for a long time before it injured the vines; but it was not so in the forests, apparently because they contained more humidity. In Burgundy it was the same as in the forest of Orleans, the underwood was injured very early. At last the frost increased so greatly that all the vines were destroyed, notwithstanding the dryness still continued; but instead of this frost doing much damage under the shelter of the wind, those parts which were sheltered were the only ones preserved, insomuch, that in many closes surrounded by walls the stocks along the southern exposition were very green, while all the rest remained dry; and in two quarters the vines were saved, the one by being sheltered from the north by a nursery of ash-trees, and the other because the vineyard was stocked with a number of fruit-trees.

But this effect is very rare, and this happened only because the season had been dry, and because the vines had resisted the weather till the plants had became so strong, from the time of the year, that the frost could not injure them, independently of the external humidity and other particular circumstances.

But there are other causes to be assigned why frost produces injury more frequently to the east than to the west, and which are drawn from the following observations:

A sharp frost causes no prejudice to plants when it goes off before the sun comes upon them; let it freeze at night, if the morning be cloudy, or a slight rain fall, or, in a word, if by any cause whatever the ice melt gently, and independently of the action of the sun, it seldom does any injury; and we have very often saved very delicate plants, which had by chance remained exposed to the frosts, by returning them into the green-house before sunrise, or by simply covering them before the sun had shone upon them.

One time in particular a very sharp frost happened in autumn while our orange-trees were out of the green-house, and as it rained part of the night they were all covered with icicles: but this accident was prevented from doing any injury by covering them with cloths before the sun rose, so that there was only the young fruit and the most tender shoots injured, and we are persuaded they would all have been saved if the covering had been thicker.

Another time our *geraniums*, and many other plants which cannot bear the frost, were out, when suddenly the wind, which was south-west, veered to the north, and became so cold that the rain, which fell abundantly, was frozen, and in almost a moment all that were exposed to the air were covered with ice; we thought, therefore, that all our plants were irrecoverably destroyed; nevertheless we had them carried to the furthermost part of the green-house, shut up the windows, and by that means they sustained but little damage.

This kind of precaution is always observed with regard to animals; when they are stricken with cold, or have a limb frozen, great care is taken not to expose them hastily to heat, but they are rubbed with snow, dipped in water, or burned in dung; in one word, the greatest attention is paid that they shall gradually be brought to warmth. It is almost certain, with respect to fruit which may be frozen, that if thawed with precipitation it invariably perishes, whereas it suffers but little if thawed gradually.

In order to explain how the sun produces so many disorders in frozen plants, some have imagined that the ice, by melting, is reduced into small spherical drops of water, which form so many small burning mirrors when the sun shines upon them. But however small the form of a mirror may be, it can only produce heat at a distance, and can have no effect on a body it touches; besides, the side of the drop of water which is on the leaf of a plant is flat, which removes its focus to a greater distance. In short, if these drops of water could produce this effect why should not the dew-drops, which are also spherical, produce the same? Perhaps, it may be thought that the most spirituous and volatile parts of the sap melting the first, they evaporate before the rest are in a state of moving in the vessels of the plant, which might decompose the sap.

But in general it may be said, that the frost increasing the volume of fluids, dilates the vessels of plants, and that the thaw cannot be performed without the parts which compose the frozen fluid enter into motion. This change may be made with sufficient gentleness not to break the most delicate vessels of plants, which will by degrees return to their natural tone, and then the plants will not suffer any injury; but, if it be done with precipitation, these vessels will not be able to resume their natural tone so soon after having suffered a violent extension, the liquors will evaporate and the plant remain dry.

Although we might conclude with these conjectures, with which I am not myself perfectly satisfied, yet the following data are irrevocably constant.

1. That it seldom happens with regard to fruit, either in spring or winter, that the plants are injured simply by the force of the frost and independently of any particular circumstances,

and when it does, it is at the northern exposition that plants meet with the greatest injury.

2. In frosty weather, which lasts several days, the heat of the sun melts the ice in some places for a few hours; for it often freezes again before sun-set, which forms an ice very prejudicial to plants, and it is observable that the southern exposition is more subject to this inconvenience than all the rest.

3. It has been observed, that spring frosts principally disorder those plants where there is humidity, the soils which transpire much, the bottoms of vallies, and in general all places which cannot be dried by the wind and sun are the most injured.

In short, if, in spring, the sun which shines on frozen plants occasion a more considerable damage to them, it is clear that it will be the eastern exposition, and those next the south which will suffer most.

But it may be said, if this be the case, we must no longer plant to the southern exposition *en a-dos* (which are slopes, or borders of earth, thrown up in kitchen gardens or along espaliers) gilliflowers, cabbages, winter lettuces, green peas, and such other delicate plants as we would have stand the winter, and preserve for an early crop in spring; and that it is to the northern exposition alone that we must in future plant peach and other delicate trees. It is proper to destroy these objections, and shew that they are false consequences of what we have advanced.

Different objects are proposed when we set plants to pass through the winter in shelters exposed to the south, and sometimes it is to expedite vegetation: it is, for example, with this intention, that along espaliers we plant ranges of lettuces, which for that reason are termed *winter-lettuces*; these will tolerably well resist the frost in whatever part we plant them, but are always most forward in this exposition; at other times, it is to preserve them from the rigour of this season, with an intention of replanting them early in the spring. This practice is also followed in winter cabbages, which are sown in this season along an espalier border. These kind of cabbages, like brocoli, are tender and cannot endure the frost, and would often perish in these shelters, if care were not taken to cover them during the sharp frosts with straw or dung supported on frames.

To forward the vegetation of some plants which will not bear the frost, as green peas, &c. it is usual for that purpose to plant them on borders exposed to the south, besides which, they are defended from sharp frosts when the weather requires it.

It is well known, without being compelled to dwell any longer on this point, that the southern exposition is more proper than all the rest to accelerate vegetation, and we have shewn that this is also what is principally proposed when some plants are set in that exposition to pass through the winter, since, in addition, we are also obliged to make use of coverings to guard those plants which are very delicate from the frost. But we must add, that if there be some circumstances wherein the frost causes more disorders to the southern than to other expositions, there are also many cases which are favourable to this exposition: for example, in winter, when there is any thing to fear from the ice, it frequently happens that the heat of the sun, increased by the reflection of the wall, has sufficient force to dissipate all the humidity, and then the plants are almost perfectly secure against the cold. Besides, dry frosts often happen, which unceasingly act towards the north, and which are scarcely ever felt towards the south. In spring, likewise, we perceive that after a rain which proceeds from the south-west, or south-east, if the wind change to the north, the southern espalier being under the shelter of the wind, will suffer more than the rest; but these cases are very rare, and most often it is after rains, which come from the north-east or north-west that the wind changes to the north, and then the southern espalier having been under shelter from the rain by the wall, the plants there will have less to suffer than the rest, not only because it will have received less rain, but also because there is always less cold here, than in other expositions. It is likewise to be observed that as the sun dries much earth along the espaliers which are to the south, the earth transpires there less than elsewhere.

It is well known that what we have just advanced must be considered as applying also to peach and apricot trees, which it is customary to put in this exposition and in that of the east. We shall only add, that it is not unusual to see peach trees frozen in the east and southern expositions, while those are not so which stand in the west or north; but notwithstanding this we can never rely on having many, nor good peaches in this last exposition, for great quantities of blossoms fall off entirely without setting; others, after having set fall from the trees, and those which remain with difficulty arrive to maturity. I have an espalier of peach-trees in a western exposition, a little a little declining to the north, which scarcely ever produce any fruit, although the trees are handsomer than those to the southern and northern. We cannot, therefore, avoid the inconveniences of the frost with respect to the southern exposition without feeling others that are worse.

All delicate trees, as fig, laurel, &c. must be set to the south, and great care taken to cover them; it is only requisite to remark that dry dung is preferable for this purpose to straw, because the latter not only does not so exactly cover them, but also from its always retaining some grain which attracts moles and rats, who sometimes eat the bark of trees to quench their thirst in frosty weather, when they can meet with no water to drink, nor herb to feed upon; and however singular this may appear, it is a circumstance which has happened to us several times; but when dung is made use of it must be dry, without which it will heat and make the young branches grow mouldy.

All these precautions are, nevertheless, very inferior to the espaliers in niches, as in that manner plants are sheltered from all winds, except the south, which cannot hurt them; the sun, which warms these places during the day, prevents the cold from being so violent during the night; and over these defended places we may put a slight covering with great facility, which will hold the plants there in a state of dryness, infinitely proper to prevent all the accidents which the spring frosts and ice might produce; and most plants will not suffer from being deprived of their external humidity, because they scarcely transpire in the winter, or in the beginning of spring, so that the humidity of the air is sufficient for their supply.

But since the dew renders plants so susceptible of the spring frost, might we not hope, that from the researches of Messrs. Musschenbroeck and Fay, some inferences may be deduced which may turn to the advantage of agriculture? for since there are some bodies which seem to attract dew, while others evidently repel it, if we could paint, plaster, or wash the walls with some matter which would have the latter effect, it is certain we should have room to expect a more fortunate success than from the precaution taken to place a plank in form of a roof over the espaliers, which cannot prevent the abundance of dew from resting on trees, since Fay has proved that it very often does not fall perpendicularly like rain, but floats in the air, and attaches itself to those bodies it encounters; so that frequently as much dew is amassed under a roof as in places entirely open. It would be easy for us to recapitulate all our observations, and continue to deduce useful consequences, but what we have said must be sufficient to shew the necessity of rooting up all trees which prevent the wind from dissipating mists.

Since by cultivating the earth we cause more exhalations to issue, great attention should be paid not to cultivate them in critical times.

We must expressly declare against sowing kitchen-plants on vine-furrows, as by their transpiration they hurt the vine.

Props should be put to the vines as late as possible. The hedges, which border them on the north side, should be kept lower than the rest. It is preferable to improve vines with mould rather than dung. And in choosing a soil we should avoid those which are in bottoms and grounds which transpire much.

A part of these precautions may be also usefully employed for fruit-trees; with respect, for example, to plants which gardeners are forward to put at the feet of their bushes and along their espaliers.

If there are some parts high and others low in a garden, we should pay attention to sow spring and delicate plants on elevated parts, at least if we do not design to cover them with glasses, &c. but in cases where humidity cannot

not hurt them it might be often advantageous to choose low places, where they might be sheltered from the north and north-west winds.

We may also profit from what has been said to the advantage of forests, for if we mean to make a reserve of any of the trees, it should never be in parts where the frost is severe; and in planting we should pay attention to put in vallies those trees which can endure the frost better than the oak.

When any considerable fall of timber is made we should make them in roads, beginning always on the north side, in order that the wind, which generally blows in frosty weather, may dissipate that humidity which is so prejudicial to the underwood.

There might be also many other useful consequences drawn from our observations; but we shall content ourselves with having briefly adverted to some, because the ingenious man may supply what we have omitted by paying a little attention to the observations we have mentioned. We are well convinced there are a great number of further experiments to be made on this matter; and perhaps even those which we have related will engage some persons to work on the same subject, and from our hints general and useful advantages may be derived.

ON THE TEMPERATURE OF THE PLANETS.

MAN newly created, and even the ignorant man at this day beholds the extent and nature of the universe only by the simple organ of light: to him the earth is but a solid body, whose volume is unbounded, and whose extent is without limits, of which he can only survey small superficial spaces: while the sun and planets seem to be luminous points, of which the sun and moon appear to be the only objects worthy regard in the immensity of the heavens. To this false idea on the extent of nature and the proportions of the universe is joined the still more disproportionate sentiment of superiority. Man, by comparing himself with other terrestrial beings, feels that he ranks the first, and hence he presumes that all was made for him; that the earth was created only to serve for his habitation, and the heavens for a spectacle; and in short the whole universe ought to yield to his necessities, and even his pleasures. But in proportion as he makes use of that divine light, which alone ennobles his being; in proportion as he obtains instruction; he is forced to abate his pretensions; he finds himself lessened in proportion as the universe increases in his ideas, and it becomes demonstrable to him, that the earth, which forms all his domain, and on which unfortunately he cannot subsist without trouble and sorrow, is as small with respect to the universe, as he is with respect to the Creator. In short, from study and application, he finds that there does not remain a possible doubt, that this earth, large and extensive as it may seem to him, is but a moderate sized planet, a small mass of matter, which, with others, has a regular course round the sun: for as it appears our globe is at the distance of at least 33 millions of leagues, and the planet Saturn at 313 millions, the natural conclusion is, that the extent of the sun's empire is a sphere, whose diameter is 627 millions of leagues, and that the earth, relative to this space, is not more than a grain of sand to the volume of the globe.

However, the planet Saturn, although the furthest from the sun, is not by any means near the confines of his empire: his limits extend much further, since comets pass over spaces beyond that distance, as may be estimated by the time of their revolutions: a comet which like that of the year 1680 revolves round the sun in 575 years must be 15 times more remote from him than Saturn; for the great axis of its orbit is 138 times greater than the distance from the earth to the sun. Hence we must still augment the extent of the solar power 15 times the distance from the sun to Saturn, so that all the space in which the planets are included is only a small province of his domain, whose bounds should be placed at least 138 times his distance from the earth.

What immensity of space! What quantity of matter! For independently of the planets, there is a probability of the existence of 400 or 500 comets, perhaps larger than the earth, which run over the different regions of this vast sphere of which the terrestrial globe only constituting a part, a unity on 191, 201, 612, 985, 514, 272, 000, a quantity represented by numbers, which imagination cannot attain or comprehend.

Nevertheless, this enormous extent, this vast sphere, is yet only a very small space in the immensity of the heavens; each fixed star is a sun, a center of a sphere equally as extensive; and as we reckon more than 2000 of these fixed stars perceived by the naked eye, and as with telescopes we can discover so much the greater number as these instruments are more powerful;

the extent of the universe appears to be without bounds and the solar system forms only a province of the universal empire of the Creator; an infinite empire like himself.

Sirius, the most brilliant fixed star, and which for that reason may be regarded as the nearest sun to our's, affords to our sight only a second of annual parrallax on the whole diameter of the earth's orbit, and is therefore at the distance of 6, 771, 770, millions of leagues distant from us, that is, 6, 767, 216 millions of leagues from the limits of the solar system, such as we have assigned it after the depth to which the comets immerse. Supposing then, there is an equal space from Sirius to that which belongs to our sun, we shall perceive that we must extend the limits of our solar system 742 times more than it is at present, as far as the aphelion of the comet, whose enormous distance from the sun is nevertheless only a unit on 742 of the total diameter of the solar system.

	LEAGUES.
Distance from the earth to the Sun - -	33,000,000
Distance from Saturn to the Sun - -	313,000,000
Distance from the aphelion of the Comet to the Sun - - -	4,554,000,000
Distance from Sirius to the Sun -	6,771,770,000,000
Distance of Sirius to the point of the aphelion of the Comet, supposing that in ascending from the sun the comet pointed directly towards Sirius (a supposition	

which diminishes the distance as much as possible)	6,767,216,000,000
One half the distance from Sirius to the Sun, or the depth of the solar and sircin system	3,385,885,000,000
Extent beyond the limits of the comet's aphelion	3,381,331,000,000
Which being divided by the distance of the comet's aphelion, gives about	742½

We can form another idea of our immense distance from Sirius, by recollecting that the sun's disk forms to our sight an angle of 32 minutes, whereas that of Sirius forms only that of a second; and Sirius being a sun like ours, which we shall suppose of equal magnitude, since there is no reason to conceive it larger or smaller, it would appear to us as large as the sun, if it were but a like distance. Taking therefore two numbers proportional to the square of 32 minutes, and to the square of a second, we shall have 3,686,4000 for the distance of the earth to Sirius, and one for its distance to the sun; and as this unit is equal to 33 millions of leagues, we see how many millions of leagues Sirius is distant from us, since we must multiply these 33 millions by 3,686,400; and if we divide the space between these two neighbouring suns, although at so great a distance, we shall see that the comets might be removed to a distance 1,800,000 times greater than that of the earth to the sun without quitting the limits of the solar universe, and without being subjected to other laws than that of our sun, and hence it may be concluded that the solar system for its diameter has an extent, which, although prodigious, nevertheless, forms only a very small portion of the heavens ; and we must infer a truth therefrom but little known, namely, that from the sun, the earth and all the other planets, the sky must appear the same.

When in a serene and clear night we contemplate all those stars with which the celestial vault is illuminated, it might be imagined that by being conveyed into another planet more remote from the sun, we should see these glittering stars larger, and emitting a brighter light, since we should be so much nearer to them. Nevertheless, the calculation we have just made demonstrates that if we were placed in Saturn, which is 300 millions of leagues nearer Sirius, it would appear only an 194,021st part bigger, an augmentation absolutely insensible ; from which it must be concluded, that the heaven, with respect to all the planets, has the same aspect as it has to the earth. Therefore if even there should exist comets whose periods of revolution might be double, or treble the period of 575 years, the longest known to us; if even the comets in consequence thereof, immerse at a depth ten times greater, there would still be a space 74 or 75 times deeper, to reach the last confines, as well of the solar system, as of the sirian; so that by allowing Sirius as much magnitude as our sun has, and supposing in his system as many or more cometary bodies than there are comets existing in the solar, Sirius will govern them as the sun governs his, and there will remain an immense interval between the confines of the two empires; an interval which appears to be no more than a desart in the vast space, and which must give a suspicion that cometary bodies do exist, whose periods are longer, and which are to a much greater distance than we can determine by our actual knowledge. Sirius may also be a sun much larger and more powerful than ours; and if that is the case, it must throw the borders of his domain so much the further back by approaching them to us, and at the same time retrench the circumference of the sun.

I cannot avoid presuming, that in this great number of fixed stars, which are all so many suns, there are some greater and others smaller than ours; others more or less luminous, some nearer, which are represented to us by those stars called by astronomers, *stars of the first magnitude*, and many others more remote, which for that reason appear to us smaller. The stars called *nebulous* seem to want light and fire, and to be only half lighted; those which appear and disappear alternately are, perhaps, of a form flattened by the violence of the centrifugal force in their motion of rotation, and are perceiveable only when they are in the full, disappearing when they are sideways. In this grand order of things, and in the nature of the stars, there are the same varieties, and the same differences, in number, size, space, motion, form, and duration; the same relation, the same degrees, and the same connection, as are found in all the other orders of the creation.

Each of the suns being endowed like ours, and like all matter, with an attractive power, which extends to an indefinite distance, and decreases, as the space increases, analogy leads us to imagine that within each of their spheres there exists a great number of opaque bodies, planets, or comets, which circulate round them, but which being much smaller than the suns which serve them for heat, they are beyond the reach of our sight.

It might be imagined that comets pass from one system to the other, and that if they happened to approach the confines of the two empires they would be attracted by the preponderating power, and forced to obey the laws of a new master. But, by the immensity of space which is beyond the aphelion of our comets, it appears that the Sovereign Ruler has separated each system by immense desarts, a thousand and a thousand times larger than all the extent of known spaces. These desarts, which numbers cannot fathom the depth of, are external and invincible barriers, that all the powers of created nature cannot surmount. To form a communication from one system to the other, and for the subjects of one to pass into the other, it would be requisite that the centre was not immoveable, for the sun, the head of the system, changing place, would draw with it in its course all the bodies which depend thereon, and hence might approach and invade another demesne. If its rout were directed towards a weaker star, it would commence by carrying off the subjects of its most distant provinces, afterwards those more interior, and would oblige them all to increase its train by revolving round it; and its neighbour thus deprived of its subjects, no longer having planets nor comets, would lose both its light and fire, which their motion alone can excite and support; hence this detached star, being no longer maintained in its place by the equilibrium of its forces, would be obliged to change nutrition, by changing nature, and becoming an obscure body, would, like the rest, obey the power of the conqueror, whose fire would increase in proportion to the number of its conquests.

For what can be said on the nature of the sun but that it is a body of prodigious volume, an enormous mass of matter penetrated by fire, which appears to subsist without aliment, and which resembles a metal or a solid body in incandescence? And from whence can this constant state of incandescence, this continually renewed production of fire proceed, whose consumption does not appear to be supported by any aliment, and whose deperdition is at least insensible, although constant for such a great number of years? Is there, or can there be, any other cause of the production of this permanent fire, but the rapid motion from the strong pressure of all bodies, which revolve round this common heat, and which heats and sets fire to it, like a wheel rapidly turned round its axis? The pressure, which they exercise by virtue of their weight is equivalent to the friction, and even more powerful, because this pressure is a penetrating power, which not only rubs the external surface but all the internal parts of the mass: the rapidity of their motion is so great that the friction acquires a force almost infinite, and consequently sets the whole mass of the axis in a state of incandescence, of light, of heat, and of fire, which hence has no need of aliment to be supported, and which, in spite of the deperdition each day made by the emission of light, may remain for ever without any sensible alteration, other suns rendering as much light to ours as it sends to them, and no part of the smallest atom of fire, or any other matter, being lost in a system where all is attracted.

If from this sketch of the great table of the heavens, and in which I have only attempted to represent to myself the proportion of the spaces, and that of the motion of bodies which travel over them; if from this point of view, to which I only raised myself to see how greatly nature must be multiplied in the different regions of the universe, we descend to that proportion of space which we are better acquainted with, and in which the sun exercises its power, we shall discover, that although it governs all bodies therein, it, nevertheless, has not the power of vivifying them, nor even that of supporting life and vegetation.

Mercury, which is the nearest to the sun, nevertheless receives only a heat 400 times stronger than that of the earth, and this heat, so far from being burning, as it has always been supposed, would not be strong enough of itself to support animated nature, for the actual heat of the sun on the earth being only $\frac{1}{50}$ part of the heat of the terrestrial globe, that of the sun on Mercury consequently is only $\frac{1}{8}$ part of the actual heat of the earth. Now if $_{7}$ parts were subtracted from the heat which is at presennt the temperature of the earth, it is certain animated nature would be checked, if not entirely extinguished. Since the sun alone cannot maintain organised nature in the nearest planet, how much more aid must it require to animate those at a greater distance? To Venus it only sends a heat $\frac{2}{50}$ times stronger than that it sends to the earth, which instead of being strong enough to support animated nature, would not certainly suffice to maintain the liquidity of water, nor perhaps even the fluidity of air, since our actual temperature would be refrigerated to $\frac{2}{49}$, which is very near the term $\frac{1}{25}$ we have given as the external limit of the slightest heat, relative to living nature. And with respect to Mars, Jupiter, Saturn, and all their satellites, the quantity of heat which the sun sends to them, in comparison with that which is necessary for the support of nature, which may be looked upon as of little effect, especially in the two larger planets, which, nevertheless, appear to be the essential objects of the solar system.

All the planets, therefore, have always been volumes (as large as useless) of matter more than dead, profoundly frozen, and consequently places uninhabited and uninhabitable for ever, if they do not include within themselves treasures of heat much superior to what they receive from the sun. The heat which our globe possesses of itself, and which is 50 times greater than that which comes to it from the sun, is, in fact, the treasure of nature, the true fund which animates us as well as every being: it is this internal heat of the earth which causes all things to germinate and to develope; it is that which constitutes the element of fire, properly called an element, which alone gives motion to other elements, and which if it was reduced to $\frac{1}{20}$ could not conquer their resistance, but would itself fall into an inertia. Now this element, this sole active power, which may render the air fluid, the water liquid, and the earth penetrable, might it not have been given to the terrestrial globe alone? Does analogy permit us to doubt that the other planets do not likewise contain a quantity of heat, which belongs to them alone, and which must render them capable of receiving and supporting living nature? Is it not greater and more worthy the idea we ought to have of the Creator, to suppose that there every where exists beings who acknowledge his power and celebrate his glory, than to depopulate all the universe, excepting the earth, and to despoil it of all beings, by reducing it to a profound solitude, in which we should only find a desart space, and frightful masses of inanimate matter.

Since the heat of the sun is so small on the earth, and other planets, it is necessary that they should possess a heat belonging solely to themselves, and our enquiry must be to see whence this heat proceeds which alone can constitute in them this element of fire. Now where shall we be able to discover this great quantity of heat if it be not in the source itself, in the sun alone? for the matter of which the planets have been formed and projected by a like impulsion will have preserved their motion in the same direction, and their heat in proportion to their magnitude and density. Whoever weighs these analogies, and conceives the power of their relations, will not doubt that the planets have issued from the sun by the stroke of a comet, because in the solar system comets only could have power and sufficient motion, to communicate a similar impulsion to the masses of matter which compose the planets. If to all these circumstances we unite that of the innate heat of the earth, and of the insufficiency of the sun to support nature, we must rest persuaded, that in the time of their formation the planets and earth were in a state of liquefaction, afterwards in a state of incandescence, and at last in a successive state of heat, always decreasing from incandescence to actual temperature, for there is no other mode of conceiving the origin and duration of this heat peculiar to the earth. It is difficult to imagine that the fire, termed central, can subsist at the bottom of the globe without air (that is, without its first aliment, and from whence this fire should proceed, which is supposed to be shut up in the centre of the globe), because what origin, what source shall we then find for it? Descartes has imagined the earth and planets were only small incrusted suns; in other words, suns entirely [illegible] extinguished. Leibnitz has not hesitated to pronounce that the terrestrial globe owes its source, and the consistence of its matters, to the element of fire: yet these two great philosophers had not the assistance of these numerous circumstances and observations which have been acquired and collected in our days, and which are so well established that it appears more than probable that the earth, as well as the planets, were projected out of the sun, and being consequently of a like matter, which was at first in a state of liquefaction they obeyed the centrifugal power, at the same time that it collected itself together by that of attraction, which has given a round form to all the planets under the equator, and flattened under the poles, on account of the variety of their rotation; that afterwards this fire being gradually dissipated, the benign temperature, suitable to organized nature, succeeded in different planets according to the difference of their thickness or density. If there should be other particular causes of heat assigned for the earth and planets, which might combine with those whose effects we have calculated, our results are not less curious, nor less useful to the advancement of science; and we shall here only observe, that those particular causes may prolong the time of the refrigeration of the globe, and the duration of living nature, beyond the terms we have indicated.

But I may be asked is this Theory equally as well founded in every point which serves for its basis; is it certain, according to your experiments, that a globe, as large as the earth, and composed of the same matters, cannot refrigerate from incandescence to actual temperature in less than 74,000 years, and that in order to become heated to the point of incandescence a 15th of this time, that is 5000 years, would be required: and also that it should be surrounded all that time by the most violent fire; if so, there are as you say strong presumptions that this great heat of the earth could not have been communicated to it from a distance, and that consequently the terrestrial matter formerly made a part of the mass of the sun; but it does not appear equally proved that the heat of this body on the earth is at present but $\frac{1}{50}$ part of the heat of the globe. The testimony of our senses seems to refute this opinion, which you lay down as a certain truth, for although we cannot doubt that the earth has an innate heat, which is demonstrated by its always equal temperature, in all deep places where the coldness of the air cannot communicate; yet does it result that this heat, which appears of moderate temperature, is greater than that of the sun which seems to burn us?

To all these objections I can give full satisfaction, but let us first reflect on the nature of our sensations. A very slight, and often imperceptible, difference in the causes which affect us, produces considerable ones in their effects. Is there any thing which comes nearer to extreme pleasure than grief? and who can assign the distance between the lively irritation by which we are moved with delight, and the friction which gives us pain? between the fire which warms and that which burns? between the light which is agreeable to our sight and that which blinds us? between the savour which pleases our taste and that which is disagreeable? between the smell of which a small quantity will at first be agreeable and yet soon after create nausea? We must therefore cease from being astonished that a small augmentation of heat, such as $\frac{1}{50}$ should appear so striking.

I do not pretend positively to assert that the innate heat of the earth is really 49 times greater than that which comes to it from the sun: for as the heat of the globe belongs to all terrestrial matter, we have no means of separating it, nor consequently any sensible and real limits to which we might relate it. But even if the solar heat be greater or smaller than we have supposed, relative to the terrestrial heat, our theory would only alter the proportion of the results.

For example, if we include the whole extent of our sensations of the greatest heat to the greatest cold, within the limits given by the observations of M. Amontons, that is, between seven and eight, and at the same time suppose that the heat of the sun can alone produce this difference of our sensations, we shall from thence have the proportion of 8 to 1 of the innate heat of the terrestrial globe to that which proceeds from the sun; and consequently the compensation which this heat of the sun actually makes on the earth, would be $\frac{1}{8}$ and the compensation which it made in the time of incandescence will have been $\frac{1}{260}$: adding together these two terms, we have $\frac{26}{900}$ which multiplied by $12\frac{1}{2}$, the half of the sum of all the terms of the diminution of heat, gives $\frac{325}{400}$ or $\frac{5}{8}$ for the total compensation made by the sun's heat during the the period of 74047 years of the refrigeration of the earth to actual temperature. And as the total loss of the innate heat is to the total compensation in the same ratio as the time of the period of refrigeration, we shall have 25 : 1 $\frac{5}{8}$: ; 74047 : 4813 $\frac{1}{25}$, so that the refrigeration of the globe of the earth instead of having been prolonged only 770 years, would have been 4813 $\frac{1}{25}$ years ; which joined to the longest prolongation, the heat of the moon would also produce in this supposition, would give more than 5000 years.

If we adopt the limits laid down by M. de Marian, which are from 31 to 32, and suppose that the solar heat is no more than $\frac{1}{32}$ of that of the earth, we shall have only $\frac{1}{4}$ of this prolongation, about 1250 years, instead of 770, which gives the supposition of $\frac{1}{50}$ which we have adopted.

But if we suppose that the sun's heat is only $\frac{1}{250}$ of that of the earth, as appears to result from the observations made at Paris, we should have for the compensation of the incandescence $\frac{1}{6250}$ and $\frac{1}{250}$ for the compensation to the end of the period of 7407 years of the refrigeration of the terrestrial globe to actual temperature, and we should find $\frac{17}{250}$ for the total compensation made by the heat of the sun during this period, which would give only 154 years, or the 5th part of 770 years for the time of the prolongation of refrigeration. And likewise, if in the place of $\frac{1}{50}$ we suppose that the solar heat was $\frac{1}{50}$ of the terrestrial, we should find that the time of prolongation would be five times longer, that is 3850 years; so that the more we endeavour to increase the heat which comes to us from the sun relative to that which emanates from the earth, the more we shall extend the duration of nature, and date the antiquity of the earth further back; for by supposing the heat of the sun was equal to the innate of the globe, we should find that the time of prolongation would be 38504 years, which consequently gives the earth a greater antiquity of 38 or 39000 years.

If we cast our eye on the table which M. de Mairan has calculated with great exactness, and in which he gives the proportion of the heat which comes to us from the sun, to that which emanates from the earth in all climates, we shall discover a well attested fact, which is, that in all climates where observations have been made, the summers are equal, whereas the winters are prodigiously unequal; this learned naturalist, attributes this constant equality of the intensity of heat in summer in all climates to the reciprocal compensation of the solar heat, and from the heat of the emanations of the central fire.

All naturalists who have employed themselves on this subject agree with me that the terrestrial globe possesses of itself a heat independently of that which comes from the sun. Is it not evident that this innate heat should be equal at every place on the surface of the globe, and that there is no other difference in this respect than that which results from the swelling of the earth at the equator, and of its flatness under the poles? A difference, which being in the same ratio nearly as the two diameters, does not exceed $\frac{1}{230}$, so that the innate heat of the terrestrial spheriod must be $\frac{1}{230}$ times greater under the equator than under the poles. The deperdition which is made, and the time of refrigeration must, therefore, have been quicker, or more sudden, in the northern climates, where the thickness of the globe is not so great as in the southern climates, but this difference of $\frac{1}{230}$ cannot produce that of the inequality of the central emanations, whose relation to the heat of the sun in winter being equal 50 to 1 in the adjacent climates to the equator, is found double to the 27th degree, triple to the 35th, quadruple to the 40th, tenfold to the 49th, and 35 times greater to the 60th degree of latitude. This cause, which presents itself, contributes to the cold of the northern climates, but it is insufficient for the effect of the inequality of the winters, since this effect would be 35 times greater than its cause to the 60th degree, and even excessive in climates nearer the poles; at the same time it would in no part be proportional to this same cause.

On the other hand there is not any foundation for supposing that in a globe which has received, or which possesses a certain degree of heat, there might be some parts of it much colder than others. We are sufficiently acquainted with the progress of heat and the phenomena of its communication, to be convinced that it is every where distributed alike, since by placing a cold body on one that is hot, the latter will communicate to the other sufficient heat to render heat of the same degree of temperature in a short time. It must not, therefore, be supposed that towards the poles there are strata of colder matters less permeable to the heat than in other climates, for of whatever nature they may be supposed to be, experience has demonstrated that in a very short time they would become as hot as the rest.

It is evident that great cold in the north does not proceed from these pretended obstacles which might oppose themselves to the issue of heat, nor from the slight difference which that of the diameters of the terrestrial spheroid must produce; but it appears to me, after much reflection upon it, that we ought to attribute the equality of the summers, and the great inequality of the winters to a much more simple cause, but which, notwithstanding, has escaped the notice of all naturalists.

It is certain that as the native heat of the earth is much greater than that which comes to it from the sun, the summers ought to appear nearly equal every where, because this same heat from the sun makes only a small augmentation to the stock of real heat which the earth possesses; and consequently if this heat issuing from the sun, be only $\frac{1}{50}$ of the native heat of the globe, the greater or less stay of it on the horizon, its greater or less obliquity on the climate, and even its total absence, would only produce one-fiftieth difference on the temperature of the climate, and hence the summers must appear, and are, in fact, nearly equal in all the climates of the earth. But what makes the winters so very unequal is the emanations of this internal heat of the globe being in a great measure suppressed as soon as the cold and frost bind and consolidate the surface of the earth and waters.

This heat which issues from the globe, decreases in the air in proportion, and in the same ratio as the space increases, and the sole condensation of the air by this cause is sufficient to produce cold winds, which acting against the surface of the earth, bind and freeze it. As long as this confinement of the external strata of the earth remains, the emanations of the internal heat are retained, and the cold appears to be, nay in fact is, very considerably increased by this suppression of a part of this heat; but as soon as the air becomes milder, and the superficial strata of the globe loses its rigidity, the heat, retained all the time of the frost, issues out in greater abundance than in climates where it does not freeze, so that the sum of the emanations of the heat becomes equal and every where alike; and this is the reason that plants vegetate quicker, and the harvest is reaped in much less time in northern countries; and for the same reason it is, that often at the beginning of summer we feel such considerable heats.

If there were any doubt of the suppression of the emanations of the internal heat by the effect of frost, we might easily be convinced of the fact; for it is a circumstance universally known, that after a frost, we may perceive snow to thaw in pits, acqueducts, cisterns, quarries, subterraneous vaults or mines, when even these depths, pits or cisterns, contain no water; the emanations of the earth having their free issue through these kinds of vents, the ground which covers this top is never frozen so strong as the open land; to the emanations, it permits their general course, and their heat is sufficient to melt the snow, especially in hollow places, at the same time that it remains on all the rest of the surface where the earth is not excavated.

This suppression of the emanations of the native heat of the earth is not only made by the frost, but likewise by the simple binding of the earth, often occasioned by a less degree of cold than that which is necessary to freeze the surface; there are very few countries where it freezes in the plains beyond the 35th degree latitude, particularly in the northern hemisphere. It appears, therefore, that from the equator, as far as the 35th degree, the emanations of the terrestrial heat having always their free issue, there ought to be in that part little or no difference between winter and summer, since this difference proceeds only from two causes, both too slight to produce any sensible effect. The first cause is the difference of the solar action, but as this action is itself much smaller than that of the terrestrial heat, its difference is too inconsiderable to be regarded as any thing. The second cause is the thickness of the globe, which towards the 35th degree, is near $\frac{1}{590}$th part less than at the equator, but even this difference can only produce a very slight effect, since at 35 degrees the relation of the emanations of the terrestrial to the solar heat is in summer from 33 to 1, and in winter from 153 to 1, which gives 186 to 2 or 93 to 1. From hence it can only be owing to the consolidation of the earth occasioned by the cold, or even to the cold produced by the durable rains which fall in these climates, that we can attribute this difference between winter and summer; the binding of the earth by cold suppresses a part of the emanations of the internal heat, and the cold, always renewed by the fall of rain, diminishes its intensity; these two causes, therefore, together produce the difference between winter and summer.

After having proved that the heat which comes to us from the sun is greatly inferior to the native heat of our globe; after having explained that, by supposing it only $\frac{1}{50}$ part, the refrigeration of the globe to actual temperature cannot be made but in 75,832 years; after having demonstrated that the time of this refrigeration would still be longer, if the heat sent from the sun to the earth were in a greater relation, namely, of $\frac{1}{25}$ or $\frac{1}{10}$ instead of $\frac{1}{50}$, we cannot be blamed for having adopted that proportion which appears the most plausible from physical reasonings, and at the same time the most probable, as it does not extend too far back the time of the commencement of nature, which we have fixed at 37 or 38,000 years, dating it from the first day.

I nevertheless acknowledge that this time, all considerable as it is, does not appear sufficiently long for certain changes, certain successive alterations, which Natural History demonstrates to have taken place, and which seem to have required a still longer course of centuries; and from which I should be inclined to imagine, that, in reality, this time would be increased perhaps double if every phenomena were completely investigated; but I have confined myself to the least terms, and restrained the limits of time as much as possible, without contradicting facts and experiments.

This theory, perhaps, may be attacked by another objection, which it is right to guard against. It may be told me that I have supposed, after Newton, the heat of boiling water to be three times greater than that of the sun in summer, and iron heated red-hot eight times greater than boiling water, that is, 24 or 25 times greater than that of the actual temperature of the earth, and that there is something hypothetical in this supposition, on which I have founded the second basis of my calculations, whose results would be, without doubt, very different if this red heat of iron, or glass in incandescence, instead of being, in fact, 25 times greater than the actual heat of the globe, were, for example only 5 or 6 times as great.

The better to feel the force of this objection, let us make a calculation of the refrigeration of the earth, upon the supposition that in the time of incandescence it was only five times hotter than it is according to our calculations; this solar heat, instead of a compensation of $\frac{1}{50}$ would have only made the compensation of $\frac{1}{250}$ in the time of incandescence, these two terms added together gives $\frac{6}{250}$, which multiplied by $2\frac{1}{2}$, the half of the sum of all the terms of the diminution of heat, gives $\frac{15}{250}$ for the total compensation which the heat of the sun has made during the whole period of the deperdition of the innate heat of the globe, which is 74047 years: therefore we shall have : $\frac{15}{250}$: : 74047 : 888 $\frac{14}{25}$ from which we see that the prolongation of refrigeration, which for a heat 28 times greater than actual temperature, has been only 770 years, should have been 888 $\frac{14}{25}$, in the supposition that this first heat should have been only five times greater than this actual temperature. This alone shews us that if we even suppose this primitive heat below 25, there would only be a longer prolongation of the refrigeration of the globe, and that alone appears to me sufficient to satisfy the objection.

It may likewise be said, " you have calculated the duration of the refrigeration of the planets, not only by the inverted ratio of their diameters, but also by the inverted ratio of their density; this might be well founded if we could imagine that in fact there exists matter whose density is as different from that of our globe: but does it exist? What, for example, will be the matter of which Saturn is composed, since its density is more than five times less than that of the earth?

To this I answer, that it would be very easy to find, in the vegetable class, matters five or six times less dense than a mass of iron, marble, hard calcareous stone, &c. of which we know that the earth is principally composed; but without quiting the mineral kingdom, and considering the density of these five matters, we have 21 $\frac{10}{72}$ for iron, 8 $\frac{25}{72}$ for white marble, for gres 7 $\frac{24}{72}$, for common marble and calcareous stone 7 $\frac{20}{72}$; taking the mean term of the densities of these five matters, of which the terrestrial globe is principally composed, we find its density to be $10\frac{5}{18}$. It is therefore required to find a matter whose density is in the relation of 189 to 1000 density, which is the same as that between Saturn and the Earth. Now this matter might be a kind of pumice stone, somewhat less dense than common pumice stone, whose relative density is here $1\frac{66}{70}$; whence it appears that Saturn is principally composed of a light matter similar to pumice stone.

So likewise the density of the Earth being to that of Jupiter as 1000 to 292, we must suppose that Jupiter is composed of a more dense matter than pumice stone, but much less dense than chalk.

The density of the Earth being to that of the Moon as 1000 to 702, this secondary planet appears composed of a matter whose density is not quite so great as that of hard calcareous stone, but more so than soft.

The density of the Earth being to that of Mars as 1000 to 730, this planet must be composed of a matter somewhat more dense than that of gres, and not so great as that of white marble.

But the density of the Earth being to that of Venus as 1000 to 12700, it may be supposed that this planet is chiefly composed of a more dense matter than emery, and less dense than zinc.

Finally, the density of the Earth being to that of Mercury : : 1000 : 2040, or : : $10\frac{5}{18}$: $20,\frac{966\frac{2}{3}}{1000}$, it must be thought that this planet is composed of a matter less dense than iron but more so than tin.

To the question, how can animated nature, which you suppose every where established, exist in planets of iron, emery, or pumice stone? I shall answer, by the same causes, and by the same means as it exists on the terrestrial globe, although composed of stone, gres, marble, iron, and glass. There are other planets like our globe, whose principal is one of these matters; but the external causes will soon have altered its superficial strata, and according to the different degrees of heat or cold, dryness or humidity, they will have converted this matter into a fertile earth proper to receive the seeds of organized nature, which only needs heat and moisture to develope itself.

Having answered the most obvious objections, it is necessary now to explain the facts, and observations, by which we are assured that the sun is only an accessory to the real heat, which continually emanates from the globe of the earth; and it will be just, at the same time, to see how comparable thermometers have taught us in a certain manner that the heat in summer is equal in all the climates of the earth, excepting Senegal, and some other parts of Africa, where the heat is greater than elsewhere.

It may be incontestibly demonstrated, that the light, and consequently the heat of the sun, emitted on the earth in the summer, is very great, comparatively with that emitted by the same body in winter; and yet, by very exact and reiterated observations, the difference of the real heat of the sun in summer is very small. This alone would be sufficient to prove that the heat of the sun makes only a small part of that of the terrestrial globe; but in addition to this M. Amontons, by receiving the rays of the sun on the same thermometer in summer and winter, observed that the greatest heat in summer in our climate differs from the cold in winter, when the water congeals, as only 7 differs from 6; whereas it can be demonstrated that the action of the sun in summer is about 66 times greater than that of the sun in winter; it therefore cannot be doubted, that there is a fund of very great heat in the terrestrial globe, on which, as a basis, the degrees of heat arise, and that at the surface it does not give a greater quantity of heat than that which comes from the sun.

If it be asked, how we can then assert that the heat in summer is 66 times grater than that in winter in our climate? I cannot give a better answer than by referring to the memoirs given by the late M. de Mairan in 1719, 1722, and 1765, and inserted in those of the Academy, where he examines, with a scrupulous attention, the vicissitudes of summers in different climates; the various causes for which may be reduced to four principal ones: 1. The inclination under which the light of the sun falls according to the different height of the sun on the horizon; 2dly. The greater or less intensity of light in proportion as its passage in the atmosphere is more or less oblique; 3dly. The different distance of the earth to the sun in summer and winter; and 4thly. The inequalities of the length of days in different climates. By the principle that heat is proportional to the action of light it will be easily demonstrated, that these four united causes, combined and compared, diminish with respect to our climate, this action of the sun's heat in a ratio of about 66 to 1 between the summer and the winter solstice; and this theoretical truth may be regarded as certain, as the second truth from experience, and which demonstrates, by the observations of the thermometer, immediately exposed to the sun's rays in winter and summer, that the difference of real heat in these two is, nevertheless, at most only from 7 to 6; I say at most, for this determination given by M. Amontons is not nearly so exact as that which has been made by M. de Mairan, who, after a great number of final observations, proves that this relation is only as 32 to 31. What, therefore, must indicate this prodigious inequality between these two relations of the action of the *solar heat*, in summer and winter, which is from 66 to 1; and of that of the *real heat*, which is only from 32 to 31? Is it not evident that the innate heat of the globe of the earth is considerably greater than that which comes to us from the sun? It appears, in fact, that in the climate of Paris this heat of the earth is 29 times greater in summer, and 491 times greater in winter than that of ths sun, as M. de Mairan has determined it. But I have already said that we must not conclude, from these two combined relations, the real one of the heat of the globe of the earth to that which comes from the sun, and I have given reasons which have determined me to suppose that we may estimate this heat of the sun 49 times less than the heat which emanates from the earth.

From the year 1701 to 1756 inclusive, a variety of observations were made with thermometers, and the following were the results. The greatest degree of heat, and of cold, which was experienced at Paris in each year was collected; a total of these was made, and it was found that the mean estimate, in all the thermometers, reduced to Rheaumur's division, was 1026, for the greatest heat in summer, that is 26 degrees above the freezing point; and that the mean degree of cold in winter, during those 56 years, was 994, or 6 degrees below the freezing point of water, whence we concluded that the greatest heat in our summers at Paris differs from the greatest cold of our winters only $\frac{1}{32}$, since 994 : 1026 : : 31 : 32; and it was on this foundation that we stated the latter to be the relation of the greatest heat to the greatest cold. But it may be objected against the precision of this valuation, the defect of the construction of the thermometer, and Rheaumur's division (to which we have here reduced the scale of all the rest); and this defect is extending only 1000 degrees below that of ice, as if 1000 degrees were in fact, that of absolute cold, whereas absolute cold

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does not exist in nature; and that of the smallest heat should be supposed 10,000 instead of 1000, which would alter the thermometer's gradation. It may likewise be said that it is possible all our sensations between the greatest heat and the greatest cold are comprised in as small an interval as that of a unit on 32 of heat, but that the voice of judgment seems to be raised against this opinion, and tells us this limit is too confined, and that it is much easier to reduce this interval than to give it an eighth, or a seventh instead of a thirty-second.

But be this valuation as it may, there can be no doubt of the truth of these facts which we have drawn from our observations, for in the same manner as we found, from the comparison of 56 successive years, the heat of summer at Paris 1026, or 26 degrees above the freezing point, we also found, with the same thermometers, that the heat in summer was 1026 in every climate of the earth, from the equator to the polar circle;* at Madagascar, in the islands of France and Bourbon, Roderigo, Siam, and the East-Indies; at Algiers, Malta, Cadiz, Montpelier, Lyons, Amsterdam, Upsal, Petersburgh, and as far as Lapland, near the polar circle. At Cayenne, Peru, Martinico, Carthagena in America; at Panama; in short, in all the climates of the two hemispheres and continents where observations could be made, it has been constantly found that the liquor of the thermometer rose equally to 25, 26, or 27 degrees in the hottest days in summer; and hence ensues the incontestible fact of the equality of heat in summer in all climates of the earth. There are indeed some exceptions, for at Senegal, and some few other places, the thermometer rises 5 or 6 degrees higher, to 31 or 32 degrees; but that arises from accidental and local causes, which do not alter the truth of the observations, nor the certainty of the general fact, which alone might demonstrate to us, that there really exists a very great heat in the terrestrial globe, that the effect, or the emanations, of which are nearly equal in all the points of its surface, and that the sun, very far from being the only sphere of heat which animates nature, is at best only the regulator. This important fact, which we consign to posterity, will enable it to discover the real progression of the diminution of the heat of the terrestrial globe, which we have been only able to determine in a hypothetical manner. In a few centuries, I am confident it will be found that the greatest heat of summer, instead of raising the liquor of the thermometer to 26, will not raise it to more than 25, or 24; and from this effect, which is the result of all the combined causes, a judgment may be formed of the value of each of the particular causes, which produce the total effect of heat on the surface of the globe; for the heat which belongs to the earth, and which it has possessed from the time of incadescence, has very considerably diminished, and will continue to diminish with the course of time: this heat is independent of that which comes from the sun; the latter may be looked upon as constant, and consequently in futurity will make a greater compensation than at present. To the loss of this innate heat of the globe there are two other particular causes, which may add a considerable quantity of heat to the effect of the two first, the only ones we have as yet taken notice of.

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* See the Memoirs of Rheaumur in those of the Academy (year 1735 and 1741), and also of the Memoirs of M. de Mairan in those of the year 1765, p. 213.

One of these particular causes proceeds, in some measure, from the first general cause, and may add something to it. It is certain that during the time of incadescence, and indeed all the subsequent ages till that of the refrigeration of the earth, not any of the volatile matters could reside at the surface, or even in the internal part, of the globe; they were raised and dispersed in the form of vapours, and could not deposit themselves but successively in proportion as it cooled, by which means some of these matters have penetrated through the clefts and crevices of the earth to great depths, in an infinity of places; and this is the primitive foundation of volcanos, which are all found in lofty mountains, were the clefts of the earth are so much the greater as these points of the globe are more projecting and isolated. This deposit of the volatile combustible matters of the first ages will have been greatly augmented by the addition of every combustible matter which has been subsequently formed. Pyrites, sulphurs, coal, bitumen, &c. have penetrated into the principal cavities of the earth, and produced almost every where great masses of inflammable matters, and often conflagrations, which have been manifested by earthquakes, erruptions of volcanos, and by the hot springs which flow from mountains, or run internally in the cavities of the earth. It may, therefore, be presumed that these subterraneous fires, some of which burn without explosion, and others with great noise and violence, somewhat increase the general heat of the globe. Nevertheless this addition of heat can be only very slight, for it has been observed that it is nearly as cold on the top of volcanos as on the top of other mountains of the same height, except at the very time when the volcano throws out inflamed vapours or burning matters.

The second cause, which seems not to have been thought of, is the motion of the moon round the earth. This secondary planet performs its evolution round the earth in 27 days and one third, and being 85,325 leagues distance, it goes over a circumference of 536,329 leagues in this space of time, which makes a motion of 817 leagues in an hour, or from 13 to 14 leagues in a minute. Although this rout is, perhaps, the slowest of all the celestial bodies, yet it is rapid enough to produce on the earth, which serves for the axis or pivot to this motion, a considerable heat by the friction which results from the weight and velocity of this planet. But it is not possible to estimate the quantity of heat produced by this exterior cause, because hitherto we have had nothing which might serve us for a term of comparison. But if we ever can discover the number, magnitude, and velocity, of all the planets which circulate round the sun, we shall then be able to judge of the quantity of heat which the moon can give to the earth, by the much greater quantity of fire which all these vast bodies excite in the sun. For my own part I am greatly inclined to think that the heat produced by this cause in the globe of the earth, forms a very considerable part of its own heat: and that, in consequence, we must still extend the limits of time for the duration of nature. But let us return to our principal object.

We have observed that the summers are very nearly equal in all climates of the earth, and that this truth is founded on incontestible facts; but it is not the same with respect to winters; they are very unequal, and vary in different climates, as we remove further from that of the equator, where the heat in winter and summer is nearly the same. I think I have already explained in a satisfactory manner the cause of this, viz, the suppression of the terrestrial heat. This suppression is, as I have said, occasioned by the cold winds, which fall from the air, bind the earth, freeze the waters, and shut up the emanations of the terrestrial heat during the time the frosts remain; so that it is not at all surprising that the cold in winter is in fact so much the greater as we advance further towards the climates where the mass of air, receiving the rays of the sun more obliquely is for that reason colder.

But with respect to the cold as well as to the heat, there are some countries which are an exception to the general rule. At Senegal, Guinea, Angola, and probably in every country where the natives are black, as in Nubia, the country of the Papous, New Guinea, &c. it is certain that the heat is greater there than in any other part of the earth; but this arises from local causes and therefore in those particular climates where the east wind reigns during the whole year, passes over a very considerable track of land, and receives a scorching heat before it arrives to them, it is not surprising that the heat is found 5, 6, and even 7 degrees greater than it is elsewhere. The excessive colds of Siberia, are also to be attributed to that part of the surface of the globe being much higher than that which surrounds it. " The northern Asiatic countries (says the Baron Strahlenberg in his description of the Russian Empire) are considerably more elevated than the European. They are like a table, in comparison of the bed on which they appear so be placed; for on coming from the west and leaving Russia, we pass to the east by the mountains Ripha and Rymnikas to enter Siberia, and constantly advance to an ascent." "There are many places in Siberia, says M. Gmelin, which are not less elevated above the rest of the earth, nor less remote from its centre, than are many high mountains in many other regions." These plains of Siberia, appear, in fact, to be as high as the summit of the Riphean mountains, on which the ice and snow do not wholly melt during summer; and if the same effect do not happen in the plains of Siberia, it is because they are less detached, for this local circumstance also adds much to the duration and to the intensity of cold and heat. A vast plain once made hot will retain its heat longer than a detached mountain, though both are alike elevated; and for the same reason the mountain once cooled will retain its snow or ice longer than the plain.

But if we compare the excess of heat with that of cold produced by these particular and local causes, we shall be surprized to find, that in Senegal, &c. where the heat is greatest, it never exceeds seven degrees beyond the summer heat in other countries, which is 26 degrees above the freezing point, while on the contrary, the colds of Siberia sometimes reach 60 or 70 degrees below it, and that at Petersburgh, Upsal, &c. under the same latitude as Siberia, the greatest cold is not more than to 25 or 26 degrees below the freezing point; therefore, we must conclude, that these local causes have much more influence in cold than in hot climates. Although we cannot pretend to determine what this great difference in the excess of cold and heat may produce, yet by reflecting on it, it appears that we may easily conceive the reason of this difference. The augmentation of the heat in such a climate as Senegal can only proceed from the action of the air, the nature of the soil, and the depression of the ground; for this country being almost on a level with the sea, it is in a great measure covered with scorching sands, over which an easterly wind continually blows; this, instead of refreshing the air, only renders it more burning, because it traverses over more than 2000 leagues of land in its way, and consequently acquires a considerable degree of heat. But in such countries as Siberia, where the plains are elevated like the summits of mountains above the level of the rest of the earth, this sole difference of elevation must produce an effect proportionally greater than the depression of the ground of Senegal, which cannot be supposed more than that of the level of the sea; for if the plains of Siberia be only elevated 4 or 500 fathoms above the level of Upsal, or Petersburgh, we must cease from being astonished that the excess of cold is so great there; since the heat which emanates from the earth, decreases at each point as the space increases, and this elevation of the ground alone suffices to explain this great difference of cold under the same latitude.

On this point there remains only one interesting question. Men animals, and plants, may, for some time, support the rigour of this cold, which is 60 degrees below the freezing point; but could they also support a heat which should be 60 degrees above it? To this we answer, yes, provided we knew as well how to guard against the heat as we do to shelter ourselves from the cold; and if the air could, during the remainder of the year, refresh the earth, in the same manner as the emanations of the heat of the globe warms the air in cold countries. We know of plants, insects, and fish, which live and grow in baths of 45, 50, and even 60 degrees of heat; there are, therefore, species in living nature which can support this degree of heat; and as the negroes are in the human race those whom a strong heat the least incommodes, might we not conclude, according to this hypothesis, that the earth has continued to decline from its original heat, and that the race of negroes are more ancient than that of white people?

GENERAL VIEWS OF NATURE.

FIRST VIEW.

NATURE is that system of laws established by the Creator for regulating the existence of bodies and the succession of beings. Nature is therefore not a body, for if it were so, it would comprehend every thing; neither is it a being, for in that case it would necessarily be God. We must rather consider Nature as an immense living power, which is in subordination to the Supreme Being, and by his command animates the universe, and whose actions are dependent on, and continued by, his concurrence or consent. This power is that part of Divine omnipotence which is manifested to mankind; it is the cause and effect, the mode and substance, the design and execution. Extremely different from all human art, whose productions are inanimate, Nature is herself a work perpetually alive, an active, an unceasing operator, who knows how to make use of every material, and whose power, though always employed on the same invariable plan, instead of suffering diminution, is perfectly inexhaustible: time, space, and matter, are her means; the universe her object; and motion and life her end.

Every object in the universe is the effect of this power. Those springs which she makes use of are active forces which time and space can only limit but can never destroy; forces which unite, balance, and oppose, but are incapable of annihilating each other. Some penetrate and connect bodies, others heat and animate them. It is principally by attraction and impulsion, that this power acts upon brute matter, while heats and organic molecules are her chief active agents, which she employs in the formation and expansion of organized beings. Aided by such instruments, how can the operations of Nature be limited? She only wants the additional power to create and annihilate to become omnipotent. But these two extremes the Almighty has reserved to himself alone; the power of creating and annihilating are his peculiar attributes; while that of changing, destroying, unfolding, renewing, and producing, are the only privileges he has conferred on this or any other agent. Nature, the minister of his *irrevocable* commands, the depositary of his *immutable* decrees, never deviates from the laws he has prescribed to her; she never changes any part of his original plan, but in all her operations she exhibits the will and design of the eternal Lord of the universe. This grand design, this unalterable impression of all existence, is the model upon which she invariably acts; a model of which all the features are so strongly impressed, that they can never be effaced; a model which the infinite number of copies, instead of impairing, only serve to renew.

We may therefore affirm that every thing has been created, but nothing annihilated; Nature acts between the two without ever reaching either the one or the other. It is in some points of this vast space, which she has filled and traversed from the beginning of ages, that we must endeavour to lay hold of her to bring her into view.

What an infinity of objects, comprehending an infinity of matter, which would have been created in vain, had it not been divided into portions, separated from each other by almost inconceivable spaces! Myriads of luminous globes, placed at immense distances, are the bases which support the fabric of the universe, and millions of opaque globes, which circulate round them, constitute the moving order of its architecture. By two primitive forces, each of which are in continual action, these masess are revolved and carried through the immensity of space; and their combined efforts produce the zones of the celestial spheres, and in the midst of vacuity establish fixed stations, and regular routes and orbits. From motion proceeds the equilibrium of worlds, and the repose of the universe. The first of these forces is equally divided, but the second is separated in unequal proportions. Every atom of matter contains the same degree of attractive force, while every individual globe has a different quantity of impulsive force assigned to each. Of the stars, some are fixed and others wandering; some globes appear formed to attract, and others to impel or be impelled. Some spheres have received a common impulsion in the same direction, and others a particular impulsion. Some stars are alone, and others are attended by satellites; some are luminous, and others opaque masses. There are planets whose different parts successively enjoy a borrowed light, and there are comets which, after being lost in the immensity of space for several ages, return to receive the influence of the solar heat. There are some suns which appear and disappear as if they were alternately kindled and extinguished; and there are others which merely shew themselves and then are seen no more. Heaven abounds with great events, which the human eye is scarcely able to perceive. A sun which expires and annihilates a world, or system of worlds, has no other effect upon the eyes of man than an *ignis-fatuus*, which gives a transitory blaze and then vanishes for ever. Man, confined to the terrestrial atom on which he vegetates, considers this atom as a world, and looks upon other worlds as atoms.

This earth which we inhabit is scarcely distinguishable among the other globes, and perfectly invisible to the distant spheres; it is at least a million times smaller than the sun by which it is illuminated, and even a thousand times less than some of the planets which, by its influence, the sun compels to circulate round him. Saturn, Jupiter, Mars, the Earth, Venus, Mercury, and the Sun, occupy that small portion of the heavens which we term our Universe. These planets, with their satellites, moving with amazing celerity in the same direction, and almost in the same plane, compose a wheel of an immense diameter, whose axis supports the whole weight, and which by the rapidity of its own rotation must inflame and diffuse heat and light throughout the whole circumference. As long as this regular motion continues (and which will be eternal, unless the Divine Mover exert the same force to destroy as He thought necessary to create them) the sun will burn and illuminate all the spheres of this universe with his splendor; and as, in a system where the whole of the bodies mutually attract each other, nothing can be lost or removed without being returned, the quantity of matter must always remain the same; this great source of light and life can never be extinguished or exhausted, for other suns, which also continually dart forth their fires, constantly restore to our sun as much light as they take from him. Comets are more numerous than planets, and like them depend on the power of the sun; they also press on the common focus, and by augmenting the weight increase the inflammation. They may also be said to form a part of our universe, for, like the planets, they are subject to the attraction of the sun. But in their projectile and impelled motions they have nothing in common either with each other or with the planets. Every one of them circulates in a different plane, and they each describe orbits in different periods of time; for some perform their revolutions in a few years, while others require several centuries. The sun, simply moving round his own centre, remains, as it were at rest in the midst, and, at the same time, serves as a torch, a focus, and an axis, to all and every part of this wonderful machine.

That the sun continues immoveable, and regulates the motions of the other globes, is to be ascribed to his magnitude alone. The force of attraction being in proportion to the mass of matter; as the sun is so considerably larger than any of the comets, and contains above a thousand times more matter than the most extensive planet, they can neither derange him nor diminish his influence, which extending to immense distances keeps the whole within the bounds of his power, and thus at particular periods recals those which have stretched furthest into the regions of space. Some of these on being brought back, approach so near the sun, that after having cooled for ages they receive an inconceivable degree of heat. From experiencing these alternate extremes of heat and cold, they are subject to singular vicissitudes, as well as from the inequalities of their motions, which at some times are most inconceivably rapid, and at others so amazingly slow as to be scarcely perceptible. In comparison with the planets the comets may be considered as worlds in disorder, for to them the orbits of the planets are regular, their movements equal, their temperature always the same; they appear to be places of rest, where, every thing being permanent, Nature, has the power of establishing a uniform plan of operation, and successively to mature her various productions. Among the planets the Earth, which we inhabit, seems to possess peculiar advantages; from being less distant from the sun than Saturn, Jupiter, and Mars, it does not experience that excess of cold; nor is it so scorched as Venus and Mercury, which appear to revolve in an orbit too near the body of that luminary. Besides, what a peculiar magnificence from Nature does the earth enjoy? A pure light, gradually extending from east to west, alternately gilds both hemispheres of this globe; which is also surrounded with a pure transparent element. By a mild and fertile heat all the germs of existence are animated and unfolded, and they are nourished and supported by a plentiful supply of excellent waters. Considerable eminences dispersed over the surface of the land, not only check, but collect the moist vapours which float in the air, and give rise to perpetual fountains. Immense cavities evidently formed for the reception of those waters, separate islands and continents. The sea in extent is equal to that of the land: nor is this a cold and barren element, but a new empire, no less rich and no less furnished with inhabitants. By the finger of the Almighty the limits of the waters are marked out. If the sea encroach on the western shores, it retreats from those of the east. This great mass of water, though inactive of itself, is agitated, and put in motion by the influence of the celestial bodies, whence arise its regular and constant flux and reflux; it rises and falls with the course of the moon, and is always at the highest when the action of the sun and moon concurs; it is from these causes uniting at the time of the equinoxes, that the tides are then higher than at any other time; and this is certainly the strongest mark of the connection of this globe with the heavens. These general and constant motions are the cause of many variable and particular circumstances; it is by those that the removals of earth are occasioned, which falling in the form of sediment, produce mountains at the bottom of the sea, similar to those which are on the surface of the land; they also give rise to currents, which following the direction of these chains of mountains, bestow on them a figure, whose angles correspond, and maintain a course in the midst of the waves as waters run upon land; they may in fact, be considered as sea-rivers.

The Air being lighter and more fluid than water, is subject to the influence of a greater number of powers. It is constantly agitated by the effects of the sun and moon, by the immediate action of the sea, and by the rarefaction and condensation of heat and cold. The winds are, as it may be said, its currents; they force and collect the clouds, they give rise to meteors, and transport the moist vapours of the ocean to the surfaces of islands and continents; from them proceed storms, and they diffuse and distribute the fertile dews and rains over the land; they interfere with the regular motions of the sea, agitate the waters, sometimes stop, and at others precipitate the currents

rents, elevate the waves, and excite dreadful storms and tempests. Forced by them the troubled ocean rises towards the heavens, and with a tremendous noise and violence, rushes against those immoveable barriers, which it can neither destroy nor surmount.

The earth being elevated above the level of the sea, it is thus defended against its irruptions. Its surface is beautifully enamelled with various flowers, and a constant renewing verdure; it is inhabited by numberless species of inhabitants, among which, man, placed to assist the intentions of Nature, presides over every other being, finds a place of perfect repose, and a delightful habitation. He alone is endowed with knowledge, and dignified with the faculty of admiration ; the Almighty has rendered him capable of distinguishing the wonders of the universe, and a witness of his increasing miracles. Animated by a ray of divinity, he participates the mysteries of the Deity. It is by this ray that he is enabled to think and reflect, and that he perceives and understands the wonderful works of his Creator.

The external throne of the Divine magnificence is Nature; and man, by contemplating her, advances by degrees to the internal throne of the Almighty. He is formed to adore his Creator, and to have dominion over every other creature; he is the vassal of heaven, and the lord of the earth; by him this nether globe is peopled, enobled, and enriched; he establishes order, subordination, and harmony among living beings, and even to Nature herself he gives polish, extension, cultivation, and embellishment; for he cuts down the thistle and the bramble, and, by his care, multiplies the vine and the rose. In those dreary desarts where man has not inhabited, we find them over-run with thorns and briars; the trees deformed, broken and corrupted, and the seeds which ought to renew and embellish the scene, are choaked by surrounding rubbish, and reduced to sterility. Nature, whom we find in other situations adorned with the splendour of youth, has here the appearance of old age and decrepitude. Here the earth, overloaded with the spoils of its productions, instead of presenting a scene of beautiful verdure, exhibits only a rude mass of coarse herbage, and trees loaded with parasitical plants, as lichens, agaries, and other impure and corrupted fruits; the low grounds are covered with putrid and stagnant waters; these miry lands being neither solid nor fluid, are not only impassable but are entirely useless to the inhabitants of both land and water; and the marshes abounding with stinking aquatic plants, serve only to nourish venomous insects, and to harbour infectious animals. There is, indeed, between the putrid marshes of the low ground, and the decayed forests of the high parts of the country, a species of lands, or savannas, but which are very different from our meadows; for in them there is an abundance of noxious herbs which spring up and check the growth of the useful kinds: instead of that delicate enamelled turf, which may be considered as the down of the earth, they are covered over with coarse vegetables and hard prickly plants, which are so interwoven, that they appear to have more connection with each other, than with the soil; and by a constant and successive generation at length form a kind of rough mat several feet thick. In these uncultivated and desolate regions, there is no road, no communication, and no vestige of intelligence. Man, when seeking to destroy the wild beasts, is compelled to follow their tracks, and to be constantly on the watch, lest he should become a victim to their savage fury; alarmed and terrified by their frequent roarings, and even awed by the profound silence of those dreary solitudes, he shrinks back and exclaims; "Uncultivated

"Nature is hideous and unflourishing; it is "I alone who can render her agreeable and "vivacious. Let us drain the marshes, and "give animation to the waters, by converting "them into brooks and canals; let us make "use of that active and devouring element, "whose power we have discovered; let us "apply fire to this burthensome load of vegetables, and to those decaying forests which "are already half destroyed; let us complete "the work by destroying with iron what cannot be removed by fire; and then instead "of coarse reeds and water-lilies, from which "the toad is said to extract his poison, we "shall soon behold the ranunculus, truffles, "and other mild and salutary herbs spring up; "that land, which was formerly impassable, "will become a flourishing pasture for flocks "of cattle, where they will find plenty of "food, and where, by the excellence of their "sustenance, they will increase and multiply, "and thus reward us for our labours and the "protection we have given them. Let us go "still further, and subject the ox to the yoke; "let his strength and weight of body be employed to plough the ground, which acquires fresh vigour from culture. Thus "will the operations of Nature be assisted, "and acquire double strength and splendor "from the skill and industry of man."

How beautiful is cultivated Nature! How lovely does she appear when decorated by the hand of man! He is himself her chief ornament, her noblest production, and by multiplying his own species he increases the most precious of her works. She even seems to multiply in proportion to his attention, for by his art he developes all that she has concealed in her bosom. What a source of unknown treasures has been brought to light! flowers, fruits and grains, matured to perfection, and multiplied to infinity; the usual species of animals transported, propagated and increased, without number; the noxious and destructive kinds diminished and driven from the habitations of men; gold, and iron a more useful metal, extracted from the bowels of the earth; torrents restrained, rivers directed in their courses and confined within their banks, and even the ocean itself subdued, investigated and traversed from one hemisphere to the other; the earth rendered active, fertile, and accesible, in every part; the vallies and plains changed into blooming meadows, rich pastures, and cultivated fields; the hills surrounded with vines and fruits, and their summits crowned with useful trees; the desarts converted into populous cities, whose inhabitants spread from its centre to its utmost extremities; roads and communications opened, established, and frequented, as so many proofs of the union and strength of society. There are besides a thousand other monuments of power and *glory*, which clearly demonstrate that man is the lord of the earth; that he has changed and improved its surface; and that from the earliest periods of time he alone has divided the empire of the world between him and Nature.

It is by the right of conquest, however, that he reigns; he rather enjoys than possesses; and it is by perpetual activity and vigilance that he preserves his advantage; if those are neglected every thing languishes, changes, and returns to the absolute dominion of Nature, she resumes her power, and destroys the operations of man; envelopes with moss and dust his most pompous monuments, and in the progress of time entirely effaces them, leaving him to regret having lost by his negligence what his ancestors had acquired by their industry. Those periods in which man loses his empire, those ages in which every thing valuable perishes, commence with war, and are completed by famine and depopulation. Although the strength of man depends solely upon the union of numbers, and his happiness is derived from peace, he is, nevertheless, so regardless of his own comforts as to take up arms and to fight, which are never-failing sources of ruin and misery. Incited by insatiable avarice, or blind ambition, which is still more insatiable, he becomes callous to the feelings of humanity; regardless of his own welfare, his whole thoughts turn upon the destruction of his own species, which he soon accomplishes. The days of blood and carnage over, and the intoxicating fumes of glory dispelled, he beholds, with a melancholy eye, the earth desolated, the arts buried, nations dispersed, an enfeebled people, the ruins of his own happiness, and the loss of his real power.

Omnipotent God! by whose presence Nature is supported, and harmony among the laws of the universe maintained; who seest from thy immoveable throne in the empyrean all the celestial spheres rolling under thy feet without deviation or disorder; who, from the bosom of repose, every instant renewest their vast movements, and who alone governs in profound peace an infinite number of heavens and of earths, restore, restore tranquillity to a troubled world! Let the earth be silent! Let the presumptuous tumults of war and discord be dispelled by the sound of thy voice! Merciful God! author of all beings, whose paternal regards extend to every created object, and to man, thy principal favourite; thou hast illumined his mind with a ray of thy immortal light; penetrate also his heart with a shaft of thy love; thy divine sentiment, when universally diffused, will unite the most hostile spirits; man will no longer dread the sight of man, nor will his hand any longer continue to be armed with murdering steel; the devouring flames of war will no longer stop the sources of generations; the human species, which are now weakened, mutilated, and prematurely mowed down, will germinate anew, and multiply without number. Nature, groaning under the pressure of calamity, sterile and abandoned, will soon resume with additional vigour her former fecundity; and we, beneficent God! shall aid, cultivate, and incessantly contemplate her operations, that we, at every moment, may be enabled to offer thee a fresh tribute of gratitude and admiration.

SECOND VIEW.

Individuals of whatever kind, or however numerous, are of no estimation in the universe; it is species alone that are existences in Nature, for they are as ancient and permanent as herself. To have a clear and distinct idea of this subject we must not consider a species as a collection or succession of similar individuals, but as a whole, independently of number or time, always active, and always the same; a whole which was considered but as one in the works of the creation, and therefore constitutes only a unit in Nature. Of these units the human species is to be placed in the first rank; all the others, from the elephant to the mite, from the cedar to the hyssop, belong to the second and third orders. Notwithstanding that they are different in form, substance, and even life, yet each sustains its appointed destination, and subsists independently of others, while the whole, in a general view, represents animated Nature, who has hitherto supported, and will continue to support, herself in the same manner as she is seen at present. Her duration is not to be estimated by a day, a year, an age, nor any given period of time, for time itself relates only to individuals, to beings whose existence is limited. It is not so with respect to species, for their existence is constant; their permanence produces duration, and their differences give rise to number. It is in this light that we must consider species, and give to each an equal right to the indulgence and support of Nature; for so *she* has certainly considered them, by bestowing on each the means of existing as long as herself.

Let us now consider the species as having changed places with the individual. In our preceding observations we have seen the relation which Nature holds in respect to man; let us now then take a view in what light she would appear to a being who represented the whole human species. We perceive that in the spring the fields renew their verdure, the buds and flowers expand, the bees revive from their state of torpor, the swallows return to our climates, the nightingale chaunts her song of love, the lamb frisks, and the bull laws with desire, and all animated creatures are eager to unite and multiply their species; and we can then have no ideas but those of reproduction and the increase of life. But when the dark season of cold and frost approaches, these same beings become indifferent to and avoid each other; many of the feathered race desert our clime, and the inhabitants of the waters lose their freedom under the massy congelations of ice; various animals dig retreats for themselves in the ground, where they fall into a state of torpor; the earth becomes hard, the plants wither, and the trees, deprived of their foliage, are covered with frost and snow; every object excites the idea of languor and annihilation. These appearances, however, of renovation and destruction, images, as it were, of life and death, although they seem general, are only individual and particular. Man, as an individual, concludes in this manner, but the being whom we have supposed as a representative of the species, thinks and judges in a manner more exalted and general; in that constant succession of destruction and renovation, and in those various vicissitudes, he perceives only permanence and duration. The different seasons in one year appear to him the same as those of the preceding, the same as those of millions of ages. The animal which may be the thousandth in the order of generation is the same to him as the first. In a word, if man had no period to his existence, and if all the beings by which he is surrounded existed in the same manner as they do at present, the idea of time would vanish and the individual would in fact become the species.

Let us then consider Nature for a few moments under this new aspect. Man certainly comes into the world enveloped in darkness. His mind is equally naked with his body; he is born without knowledge and without defence, and brings nothing with him but passive qualities. He is compelled to receive the impressions of objects on his organs; even the light shines on his eyes long before he is able to recognize it. To Nature he is at first indebted for every thing, without making her any return. No sooner, however, do his senses acquire strength and activity, and he can compare his sensations, than he reflects upon the universe; he forms ideas, which he retains, extends, and combines. Man, after receiving instruction, is no longer a simple individual, for he then, in a great measure, represents the whole human species. He receives from his parents the knowledge which had been transmitted to them from their forefathers; and thus, by the divine arts of writing and printing, the present age, in some sort, becomes identified with those that are past. This accumulation of experience in one man, almost extends the limits of his being to infinity. He is born no more than a simple individual, like other animals, capable only of attending to present sensations; but he becomes afterwards nearly the being which we supposed to represent the whole species; he reads what has past, sees the present, and judges of the future; and in the torrent of time, which carries off and absorbs all the individuals of the universe, he perceives that the species are permanent, and Nature invariable. As the relations of objects are always the same, to him the order of time appears to be nothing; he considers the laws of renovation as only counterbalancing those of permanency. An uninterrupted succession of similar beings, is, in effect, only equivalent to the perpetual existence of one of them.

What purposes then are gained by this immense train of generations, this profusion of germs, many thousands of which are abortive for one that is brought into life? Does not this perpetual propagation of beings, which are alternately destroyed and renewed, uniformly exhibit the same scene, and occupy the same proportion in Nature? From what cause proceed all these changes of life and death, these laws of growth and decay, all these individual vicissitudes, and reiterated representations of the same identical thing? They certainly arise from the very essence of Nature, and depend on the first establishment of the universal machine; the whole of which is fixed and stable, but each of its parts being endowed with the power of motion, the general movements of the celestial bodies have produced the particular ones of this terrestrial globe. The penetrating forces by which these immense bodies are animated, and by which they act reciprocally upon each other at a distance, at the same time animate every particle of matter; and this strong propensity, which every part has towards each other, is the first bond of beings, the ground of consistence and permanency in Nature, and the support of harmony in the universe. From these great combinations the smaller relations are derived. The earth moving on its own axis having separated the portions of duration into day and night; all its animated inhabitants have their stated periods of light and darkness, of their times of waking and sleeping. The action of the senses, and the motions of the members which form a great part of the animal economy, are related to this first combination; for in a world where perpetual darkness reigned, would there be senses alive to the enjoyment of light.

As the inclination of the axis of the earth, in its annual course round the sun, produces considerable variations of heat and cold, which we call seasons, all its vegetables have also, either wholly or partially, their seasons of life and death. The fall of the leaves, and the decay of fruits, the withering of herbs and the destruction of insects, depend entirely on this second combination. In those climates where there is not this variation, by the inclination not being so material, the life of the vegetable is not suspended, and every insect completes the stated period of its existence. Where the four seasons, in fact, make but one, as under the line, the surface of the earth is constantly covered with flowers, the trees have a perpetual foliage, and Nature seems to enjoy a continual spring.

Both in animals and plants, their particular constitution is relatively to the general temperature of the earth, and which temperature depends upon its situation and distance from the sun. If they were removed to a greater distance, neither our animals, nor our plants, could live or vegetate; the water, sap, blood, and all their liquors, would lose their fluidity; if on the contrary they were more near they would vanish and dissipate in vapour. Ice and fire are the elements of death, and temperate heat the first support of life. The living particles so generally diffused through all organized bodies are related, not only by their activity but number, to the particles of light which strike and penetrate almost all matter with their heat; for in every place where the sun can heat the earth with its rays, the surface will be covered with verdure, and peopled with animals; even ice is no sooner dissolved into water than it swarms with inhabitants. Water, indeed, is apparently more fertile than the earth; from heat it receives motion and life. In one season the sea produces more animals than the earth sustains; but its production of vegetables is infinitely less. And because that the inhabitants of the ocean have not a a sufficient and permanent supply of vegetables, they are compelled to feed upon each other; and it is to this necessity that their immense multiplication may be referred.

As in the beginning every species was created, the first individual of each has served for a model to their descendants. The body of each animal or vegetable is a mould, to which are assimilated indifferently the organic particles of all animals or vegetables which have been destroyed by death, or consumed by time. The brute particles, of which part of their composition was formed, returned to the common mass of inanimate matter; but the organic particles, whose existence is permanent, are again resumed by organized bodies: they are extracted at first from the earth by vegetables, and then absorbed by animals who feed thereon; and thus serve for the support, growth, and expansion of both. By this constant and perpetual circulation from body to body, they serve to animate all organized beings. These living substances in quantity are always the same, and differ only in form and appearance. In fertile ages, and when population is the greatest, the whole surface of the earth seems to be covered with men, domestic animals, and useful plants. But in the times of famine and depopulation, the ferocious animals, poisonous insects, parasitical plants, and useless herbs, resume, in their turn, dominion over the earth. To man these changes are material, but to Nature they are perfectly indifferent. The silk worm so inestimable to the former, is to the latter only a caterpillar of the mulberry tree. Though this caterpillar, which so materially assists in the supply of our luxuries, should disappear; though the plants, from which our domestic animals procure their nourishment, should be devoured by other caterpillars; though still others should destroy the substance of our corn before the harvest; in short, though man and the larger animals should be starved by the inferior tribes, Nature would not be less abundant nor less alive; she never protects one at the expence of another, but especially supports the whole. As to individuals she is regardless of number; she considers them only as successive images of the same impression; as passing shadows of which the species is the substance.

In earth, air, and water, then, there exists a certain quantity of organic matter which cannot be destroyed, but which is constantly assimilated in a certain number of moulds, that are perpetually undergoing destruction and renewal: these moulds, or rather individuals, tho' varying in number in every species, are nevertheless always the same, that is, proportioned to the quantity of living matter; and this appears to be absolutely the case, for if there were any redundance of this matter, or if it were not at all times fully occupied by the individuals of the species which exist, it would, most assuredly, form itself into new species, for being alive it would not remain without action; and once uniting with brute matter is sufficient to form organized bodies; and it is by this constant combination, and invariable proportion, that Nature preserves her form and consistence.

The laws of Nature, both with respect to the number of species and of their support and equilibrium, being fixed and constant, she would invariably have the same appearance, and be in all climes absolutely the same, if her complexion did not so completely vary in almost every individual form. The figure of each species is an impression, in which the principal characters are so strongly engraven as never to be effaced; but the accessory parts and shades are so greatly varied that no two individuals have a perfect resemblance to each other; and in all species there are a number of varieties. The human species, which has such superior pretensions, varies from white to black, from small to great, &c. The Laplander, the Patagonian, the Hottentot, the European, the American, and the Negro, though the offspring of the same parents, have by no means the resemblance of brothers.

It is evident, therefore, that every species is subject to individual differences, but that each of them does not equally possess the constant varieties which are perpetuated through successive generations; the more dignified the species, the less changeable is its figure, and the less are the varieties of it. The multiplication of animals being inversely in proportion to their magnitude, as the possibility of variation must be in exact proportion to the numbers they produce, there consequently must be more varieties among the small than the large animals; and also, for the same reason, there will be a greater number of species which seem to approach each other; for the unity of the species in the large animals is more fixed, and the nature of their separation more extended. What a number of various and similar species surround those of the squirrel, the rat, and other small quadrupeds, while the massy elephant stands alone, without a compeer, and at the head of the whole.

The brute matter, of which the body of the earth is principally composed, is a substance that has not undergone many alterations, though the whole has more than once been disturbed and put in motion by the hand of Nature. The globe of the earth has been penetrated by fire, and afterwards covered and disordered by water. The sand, which occupies the interior parts of the earth, is a vitrified matter; and the layers of clay, by which its surface is covered, are nothing but the same sand having been decomposed by the operation of the waters. Granite, free-stone, flint, nay, all metals, are composed of this same vitrified matter, whose particles have been condensed or separated, according to the laws of their affinity. These substances are totally destitute of animation; they exist, and will continue to do so, independently of animals and vegetables. There are, however, many other substances, which, although they have the appearance of being equally inanimate, originate from organized bodies; and of this description are marble, lime-stone, chalk, and marl; they being composed of the fragments of shells, and of those small animals which by transforming the water of the sea into stone, produce coral, and all the madrepores, whose varieties are numberless, and whose quantity are almost immense. Pit-coal, turf, and many other substances found in the upper strata, are also of this nature, they being only the residue of vegetables which have been more or less corrupted or consumed.

Besides these, there are other substances which have been produced by the second action of fire upon original matter; these are but few in number, and consist of such as pumice-stones, sulphur, the scoria of iron, asbestos, and lava. To one or other of these three great combinations may be referred all the relations of brute matter, and all the substances of the mineral kingdom.

The laws of affinity, by which the various particles of these different substances separate from each other, in order to unite among themselves and form homogeneous masses, are perfectly similar to that general law by which the celestial bodies act upon each other; in both cases their exertions are the same. Globules of water, of sand, or of metal, have the same influence, and act upon each other as the earth acts upon the moon; and if the laws of affinity have hitherto been deemed different from those of gravity, it is because the subject has been considered in a very confined point of view. The mutual action of celestial bodies is very little influenced by figure; their distance from each other is so very great, that this is necessarily the case; but when they are not far asunder, then the effect of figure is considerable. For instance, if the earth and moon, instead of spherical figures, were both short cylinders, and exactly equal throughout in their diameters, their reciprocal action would be very little varied from what it is at present, because the distances of all their parts from each other would be very little changed. But if these two globes were cylinders of great extent, and approached near to each other, the law of their reciprocal action would seem to be different, inasmuch as the distances of their parts would be greatly varied; and hence whenever figure becomes a principle in distance the law will appear to vary, although in fact it is always the same.

The human intellect guided by this principle, may advance one step further in penetrating into the operations of nature. The figure of the constituent particles of bodies still remains unknown; we cannot entertain the smallest doubt that water, air, earth, metals, and all homogeneous particles, are composed of elementary particles, which are perfectly similar, although we are still ignorant of their figure. By the aid of calculation this at present unknown field of knowledge may be disclosed by posterity, and the figure of the elementary bodies be ascertained with tolerable precision. They may take the principle we have established as the basis of their enquiry;

namely, "that all matter is attracted in the "inverse ratio of the square of the distance; "and this law seems to admit of no variation "in particular attractions but what arises from "the figure of the constituent particles of each "substance, because this figure enters as an "element or principle into the distance;" and having once discovered, by repeated experiments, the law of attraction in any particular substance, they may then, by the aid of calculation, be able to trace the figure of its constituent particles. To render this point more clear, let us suppose, that by placing mercury on a perfectly polished surface, repeated experiments prove that this fluid metal is always attracted in the inverse ratio of the cube of the distance; it will then become necessary to investigate what figure gives this expression; and this figure will be certainly that of the constituent particles of mercury. If it should appear, by such experiments, that the attraction of mercury was in the inverse ratio of the square of the distance, it would be clearly demonstrated that its constituent particles were spherical, because a sphere is the only figure which observes this law, and at whatever distance globes are placed the law of their attraction is always the same.

Newton had some idea that chemical affinities (which are nothing more in fact than these particular attractions which we have mentioned) were produced by the same kind of laws as those of gravitation; but he does not appear to have perceived that all those particular laws were merely simple modifications of the general one, and that their apparent difference arose solely from the circumstance of the figure of the atoms, which attract each other, having, when at small distances, a greater influence upon the force of this law than the mass of matter.

It is, notwithstanding, upon this theory that the perfect knowledge of brute matter depends. The basis of all matter is the same, and its form throughout would be perfectly similar, if the figures of its constituent particles were not different; and thus it is that one homogeneous substance can differ from another only in proportion to the difference of their original particles. A body composed of spherical particles ought to be one half specifically lighter than that whose particles are cubical, because as the first only touch each other by their points, they leave intermediate spaces equal to what they occupy, whereas the cubical particles join without leaving the smallest interval, and must consequently form a matter half as heavy again. Although the figures are considerably varied, that variation is by no means so great as we might imagine, since Nature has fixed the limits of lightness and gravity. Gold and air, with respect to density, are the two extremes, and therefore all the figures in Nature must be comprehended as coming between those two; such as would have produced heavier or lighter substances have been rejected.

In speaking of figures, as employed by Nature, I do not mean to imply that they must be necessarily, or are exactly, similar to those geometrical figures which we form in our imagination. We form laws by supposition, and then endeavour to render them simple by abstraction. It is very possible that there are neither exact cubes nor perfect spheres in the universe; but as nothing certainly exists without form, and as from the variation of substances the figures of the elements are different, some of them most undoubtedly must approach to the sphere, the cube, and all the other regular figures which we have conceived. The precise, absolute, and abstract figures which our minds are so frequently induced to admit, cannot have any existence, because all objects are related, and differ only by almost imperceptible shades. It is by the same rule that when I speak of one substance as being entirely full, because composed of cubical particles, and another as being not more than half full, because its parts are spherical, I mean only comparatively, and not that such substances really exist; for experience has fully informed us, that in transparent bodies, such as glass, which is both dense and heavy, there is but a small quantity of matter in proportion to the extent of the intervals; nay, as we have before observed, it might be demonstrated that even gold, which is the most dense species of matter, has more vacuities than substance.

To investigate the powers of Nature is the object of rational mechanics, while active mechanics is solely confined to a combination of particular powers, and consequently the art of constructing machines. This art has at all times been certain of cultivation from necessity and convenience; and both ancients and moderns have equally excelled in it. But rational mechanics is a science invented in our days; for, from the days of Aristotle to those of Descartes, even the philosophers have reasoned no better upon the nature of motion, than uniformly to mistake the effect for the cause. Impulsion was the only force with which they were acquainted; to it they attributed the effects of others, and all the phenomena of the universe. If this idea of theirs had been probable, or even possible, impulsion, which they regarded as the sole cause, must have been a general effect, which equally belonged to all matter, and which equally exerted itself in all places, and at all times; but every day demonstrated the contrary to be the fact; for they must have perceived that this force had no existence in bodies at rest; that it had but a short subsistence in projected bodies, being soon destroyed by resistance; that a fresh impulse was absolutely necessary for its renewal, and that, consequently, so far from being a general cause, it was only a particular effect produced by others more general.

It is true, however, that we ought to consider a general effect as a cause, for we cannot become acquainted with the real cause of this effect, because all our knowledge is derived from comparison; and as there is not any thing to which we can compare an effect, which is supposed general, and equally belonging to every thing, we can know it only by the fact. According to this view, attraction, or gravity, being a general effect common to all matter, and clearly evinced by the fact, ought to be considered as a cause; and to which all particular causes should be referred, nay even that of impulsion, since it is less general and less constant; and the principal difficulty is to perceive how impulsion can be an effect of attraction; for if we rest on the communication of motion by impulse, we are then persuaded that it can only be transmitted from one body to another by elasticity, and that all the hypotheses, which suppose a communication of motion in hard bodies, are mere ideal fancies, which do not exist in Nature. A perfectly hard or a perfectly elastic body is entirely imaginary, as neither of them really exist; for it is certain that nothing exists absolutely or in extreme; and the idea of pertion must suppose one or the other.

It is certain that if there were no elasticity in matter there would be no impulsive force; for instance, if we throw a stone, the motion it acquires is communicated by the elasticity of the arm. When motion is communicated by one body in action encountering another at rest, how can we possibly suppose it to be done otherwise than by compressing the spring of the elastic particles it contains, which recovering itself almost immediately after, gives to the whole mass a force equal to that which it received?

ceived? How a perfectly hard body should admit this force, or receive motion, is beyond comprehension; and the enquiry is unnecessary, since no such body exists; for, all bodies are endowed with elasticity. The force of electricity is proved by experiments to be elastic, and to belong to matters in general; and therefore, if no other elasticity existed in the interior parts of bodies but that of this electrical matter, that would be sufficient for the communication of motion; and consequently to this great spring, as a general effect, the particular cause of impulsion must be attributed.

A little reflection on the mechanism of elasticity will convince us that its force depends on that of attraction. To have a still more clear idea of this subject, let us suppose a spring the most simple, such as of a solid angle of iron, or of any other hard substance, and then see what will be the result of compressing it. By compression we oblige the parts adjacent to the top of the angle to bend, or to separate a little from each other; but the pressure being removed they again approach as near as they had done before. Their adhesion, from which the cohesion of bodies results, is clearly an effect of their mutual attraction. Upon the spring being pressed this adhesion is not destroyed, because, although the particles are separated, they are not removed beyond the sphere of their mutual attraction; consequently the moment the pressure is taken away the force is renewed, the separated parts draw near, and their spring is restored. But if the pressure be too violent, they will, in that case, be removed beyond the sphere of their attraction, and the spring will break, because the compressing force will be greater than that of cohesion, or that of mutual attraction, by which the particles are kept together. This proves that elasticity can only exert itself in proportion to the cohesion of the particles of matter, that is, in proportion as they are united by the force of their mutual attraction; from which it results, that elasticity in general, which alone can produce impulsion, and impulsion itself, are owing to the force of attraction, and are only particular effects which depend on that general one.

Notwithstanding that these ideas appear to be perfectly clear to me, I do not expect to see them adopted. People in general reason only from their sensations, and natural philosophers determine from their prejudices; as, therefore, both these must be set aside, very few will remain to form a proper judgment; but such is the dignity of Truth, that she is content with a few admirers, and is always lost in a crowd; she is at all times august and majestic, notwithstanding which she is frequently obscured by fantastic opinions, and obliterated by fanciful chimeras. I, however, view and understand Nature in this manner, and am almost induced to believe that she is still more simple; the phenomena exhibited by brute matter is caused by a single force, and from this force, combined with that of heat, originate those living particles which gave rise to, and support all, the various effects of organized bodies.

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